CENTENARY PROCEEDINGS

OF THE

ROYAL METEOROLOGICAL SOCIETY

1950

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CENTENARY PROCEEDINGS OF THE ROYAL METEOROLOGICAL SOCIETY

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Foreword

This Centenary Volume records some parts of the proceedings in which we celebrated the hundredth birthday of our Society. I hope that these cool clear pages of modern type on modern paper — and in both these things, in spite of all the pessimists' talk of the deficiencies of our age, we do better than we did a hundred years ago — do not wholly conceal the warm friendly informalities and the hot scientific enthusiasms of our celebrations.

We looked backwards with pious gratitude, with a keen sense of the remarkable transformation which had been wrought within the century, of the far-seeing diligence which had provided observational and conjectural springboards for the half-dozen triumphant leaps forward that had made our science and our art of meteorology what it is to-day. We did honour to the dramatic peaks of physical and intellectual courage and to the undramatic levels of disciplined devotion to a schedule, which were equally necessary and important to the transformations and the progress. We expressed our legitimate pride in the British contribution to the advance, we expressed a still higher pride in that very early united nations organisation which worked so smoothly and quietly in meeting the universal needs for dependable records of past weather and climate and for some forewarning of coming weather.

We looked forward with a no less keen sense of our responsibilities, our partial failures, our gravely incomplete successes. We admitted frankly but without discouragement that we had chosen to cultivate a singularly intractable field of study and learning. We had tilled, spade deep, a patch of back-garden, only to discover that the sky was not the limit, that the whole terrestrial sphere was our parish, that the solar system at least was necessary to the flowering of the back-garden.

It is a legitimate criticism that in the scientific symposia in particular we attempted too much. It is a legitimate reply that the limit of the possible is discovered — and extended — only by a confident assault on the impossible. The organisational problems of the symposium were a miniature working model of the scientific problems of meteorology ; every section of the discussions interfered disconcertingly with the timetable of every other ; but the content of every section was indispensable to and inseparable from that of every other. The satisfaction felt in the miraculously smooth running of the symposia, despite these healthy jostlings among the exuberantly adolescent subjects, is a reliable forecast of success in the forwarding of that science of meteorology which is one and indivisible yet can only advance by temporary subdivision en route to ultimate synthesis. If the seaman thinks we paid too little attention to the sea, it was not because we were under-conscious of the importance of weather to seamen or of the influence of the sea on weather, it was because we were acutely conscious that the seaman's weather is manufactured, in large measure, in the less accessible regions of the airman's medium. All that we do for the airman, all that the airman does for us, is essential to our better service to the seaman and to our extracting fuller value from his service to us.

There are other ways in which the centenary celebrations, and among them this lasting embodiment of the celebrations, the Centenary Volume, are a microcosm of meteorology itself. The numerous, brilliant, stimulating and most welcome contributions from a galaxy of distinguished friends and visitors from overseas which are reproduced (in so far as champagne can be reproduced in print) re-emphasised that fact that if the weather belongs to everybody it is everybody's responsibility, that if the world is our parish, the parishioners in the Americas, in Scandinavia (including Little Scandinavia in America) and in all the continents, are making their contribution to the parish rates and were responsible in great measure for the success of this particular Parish Meeting. Nor is the Iron Curtain weatherproof ; we are fortunate, and the world is fortunate, in its high two-way transmission coefficients for meteorological information and meteorological science. This volume bears the imprint Royal, but it is the work of a world republic of meteorological science.

An outstanding impression from the meetings is the magnitude of the contributions to the " geography " of our meteorological world which are recorded here. The revisions of the maps of the general circulation which are always impending and always in progress brought us such outstanding contributions as those of Rossby and of Petterssen ; I may be allowed to speak specially of the amount of fresh thought and effort that will be stimulated by Petterssen's charts of cyclogenesis, and of Rossby's salutary reminder of the acute difficulty of attaining even a rudimentary laboratory model of even the most prominent feature of the general circulation.

And that we met in libraries, laboratories and lecture rooms, that we made personal observations of the climatology of colleges, that we made our way to work through departments of mathematics, of physics, of chemistry and of biology, that we found the moving picture an invaluable tool and a stimulating synoptic medium — all these are of the microcosm, for in physics alone we found ourselves faced with a diversity of phenomena in radiation, in cloud physics, in fluid dynamics and so on which is worthy of the steel of a whole *corps d'élite* of first-rank physicists : if only the poor misguided creatures could be made aware of the importance and the allurements of weather and its science !

The allurements — perhaps that was the sharpest impression of all. Meteorology is always interesting, but now, it seems to me, we have the right, with the most critical consideration, to declare that it revealed itself in these celebrations as being in a most exciting phase — the answers to century-old questions are just beginning to be discernible through the mist of still incomplete observation, tools worthy of the awesome magnitude of our physical problems are just being forged, we are impressed but not depressed with the size of the job we have taken on, we are modest but confident.

So, forward to our second century ! And in a truer sense than that intended by the writer of the last song-hit but one

" Enjoy yourself, it's later than you think."

ROBERT WATSON-WATT

Introduction

ROYAL METEOROLOGICAL SOCIETY — CENTENARY CELEBRATIONS 1950

This volume contains the record of the Centenary Celebrations of the Society held at Oxford and London between Tuesday 28 March and Monday 3 April, 1950.

Some 350 members and guests, including 60 visitors from 24 countries, took part in the celebrations which were arranged by a Committee of Council consisting of :

CENTENARY COMMITTEE

Chairman—Sir Robert Watson-Watt, C.B., F.R.S.

Secretary—O. M. Ashford, B.Sc., A.Inst.P.

F. J. Scrase, O.B.E., M.A., Sc.D., F.Inst.P.
J. M. Stagg, O.B.E., M.A., D.Sc.
R. C. Sutcliffe, O.B.E., Ph.D.
W. M. Witchell, B.Sc.

Symposia Sub-Committee

O. M. Ashford, B.Sc., A.Inst.P.
P. A. Sheppard, B.Sc., F.Inst.P.
R. C. Sutcliffe, O.B.E., Ph.D.

Oxford Representative—A. W. Brewer, Ph.D., A.Inst.P.

The programme of events was as follows :—

28-31 March 1950. Meteorological Symposia at Clarendon Laboratory, Oxford. By kind permission of Lord Cherwell, F.R.S.

Tuesday 28 March (afternoon). " Radiation and its effect on the troposphere and lower stratosphere."

Wednesday 29 March (morning). " Physics of cloud and precipitation."

Wednesday 29 March (afternoon). " The structure of weather systems."

Thursday 30 March (morning). " The general circulation."

Thursday 30 March (afternoon). " Climatic change."

Friday 31 March (morning). " Meteorology and the community."

1 April 1950 (afternoon). Visit to Meteorological Office, Harrow, Middlesex, by kind invitation of the Director, Sir Nelson K. Johnson, K.C.B.

The work of the Climatological, Instrument and Marine Branches was displayed, a forecasting unit was shown at work and a radio-sonde ascent was demonstrated.

1 April 1950 (evening). Conversazione in the Society's rooms at 49 Cromwell Road, London.

A programme of music was played by The Occasional Wind Players (Director : G. E. W. Hartley, M.A., A.R.C.M.), and the following films were shown :—

A trip in an ocean weather ship (O. M. Ashford),

Some laboratory and field observations related to meteorology (Dr. V. J. Schaefer),

The International Colorado Beetle Campaign (Messrs. Plant Protection Ltd.)

3 April 1950 (morning). Symposium on "Atmospheric turbulence" in the Department of Meteorology, Imperial College of Science and Technology, London, by kind invitation of Sir David Brunt, F.R.S. The department was open to inspection after the symposium.

3 April 1950 (afternoon). Centenary Meeting of the Society at 49 Cromwell Road, London.

3 April 1950 (evening). Centenary Dinner at the Connaught Rooms, London, W.C.2.

RELATED EVENTS

Exhibition, "The Science of Weather" at the Science Museum

The Director of the Science Museum presented an exhibition under the above title from 28 March to 26 June to mark the Society's Centenary. The exhibition was in five sections : *A century of meteorology* ; *Observing the weather* ; *Forecasting the weather* ; *Weather in our daily lives* ; *Meteorological research*, and is described in the official handbook, *The science of weather* (1950, H.M.S.O.).

Physical Society Exhibition

A special place was accorded to meteorology in the Physical Society's annual Exhibition of Scientific Instruments held at Imperial College of Science and Technology, London from 31 March to 3 April 1950. Evening discourses were given by Dr. F. J. Scrase (*Meteorological Office*, London) on "Sounding the upper air" and by Dr. V. J. Schaefer (*G.E.C. Research Laboratories*, Schenectady, U.S.A.) on "Experiments with cloudy skies."

Radio broadcast by the British Broadcasting Corporation

A broadcast entitled *A hundred years of weather* was given on the Home Service wave-lengths on Wednesday 5 April 1950, from 9.15 to 10 p.m., from the Society's rooms at 49 Cromwell Road, London.

The President, Sir Robert Watson-Watt, gave talks on "100 Years of weather watching" and "The earth's atmosphere" on 29 March and 2 April respectively.

CENTENARY SYMPOSIUM
ON
RADIATION AND ITS EFFECT ON THE TROPOSPHERE AND LOWER STRATOSPHERE

Held at Oxford on the afternoon of 28 March 1950
Chairman : Sir ROBERT WATSON-WATT, C.B., F.R.S.

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The emission spectra of the earth's surface, the troposphere, and the lower stratosphere

By ARTHUR ADEL
Arizona State College

Summary

Simultaneous observations of the infra-red emission spectra of the earth's surface, the troposphere, and lower stratosphere have been obtained at Flagstaff, Arizona (1941) and Alamogordo, New Mexico (1948). In conjunction with atmospheric absorption spectra they lead, *via* simple calculations, to the effective radiation temperatures of the ozone and nitrous oxide layers :

Ozone	—40°C
Nitrous oxide	+ 5°C

1. Introduction

An experiment which has long been of interest to meteorologists is the one which reveals the radiation exchange between the earth's surface and its atmosphere. The present paper considers an elaboration of this experiment in which the radiation exchanges are resolved into their spectra. This makes possible the calculation of the effective radiation temperatures of the polyatomic constituents of the troposphere and lower stratosphere. The experiment is idealized to the extent of allowing the earth's surface to be represented by the receiving surface of a thermocouple. The atmosphere participates directly.

Such an experiment is by its very nature also an examination of the low dispersion absorption and emission characteristics of the polyatomic constituents of the troposphere and lower stratosphere. (In the absence of polar diatomic molecules the polyatomic molecules are the only ones which can capture surface radiation, and thus participate in an exchange.) However, only those infra-red bands lying at wave-lengths longer than four microns are involved, since thermal radiation at even the highest temperatures of the earth's surface and any substantial part of its atmosphere is inappreciable at wave-lengths shorter than four microns. It is, of course, true that about 99 per cent of the solar radiation arriving at the earth is located at wave-lengths shorter than four microns, and this is indeed one of the boundary conditions defining the problem of the radiation equilibrium of our planet. The essential idea of the present account, however, concerns one of the other boundary conditions of the radiation equilibrium problem ; namely, the one which determines the initial stages of the return journey of the greatly degraded energy to space. The latter boundary condition is an expression of terrestrial temperatures and of the presence of comparatively minute but important amounts of polyatomic compounds in the troposphere and lower stratosphere (Table I).

2. THE EXPERIMENT

The experimental process of registering concurrent emission spectra of the earth's surface and the polyatomic constituents of the troposphere and lower stratosphere is straightforward. A conventional infra-red spectrometer equipped with a rock salt or potassium bromide prism is employed. The place of the customary source of infra-red radiation is taken by a train of mirrors, including a coelostat, by way of which the thermocouple views the sky through the prism and is simultaneously viewed by the sky.

The thermocouple receiver may be regarded as a bit of the earth's surface radiating to the sky as a near-black body at room temperature. The normal spectrum of the escaping radiation is thus peaked at ten microns, drops sharply to an almost negligible intensity at four microns, and fades slowly from ten microns toward the longer wave-lengths.

TABLE I. POLYATOMIC CONSTITUENTS OF THE EARTH'S ATMOSPHERE

Compound		Amount
Water vapour (H_2O)		From 1/4 mm to more than 50 mm liquid water layer equivalent
Carbon dioxide (CO_2)	~	3 m N.T.P.
Ozone (O_3) ..	~	3 mm N.T.P.
Nitrous oxide (N_2O)	~	5 mm N.T.P.
Methane (CH_4) ..	~	1 cm N.T.P.
Heavy water (HDO)	~	10^{-6} of H_2O vapour

The net loss of energy from the earth's surface is reduced beneath the value of the above spectrum by the arrival of the characteristic radiations from the atmosphere, principally those of H_2O, CO_2, O_3, N_2O, and CH_4. This net result is shown in Figs. 1 and 2.

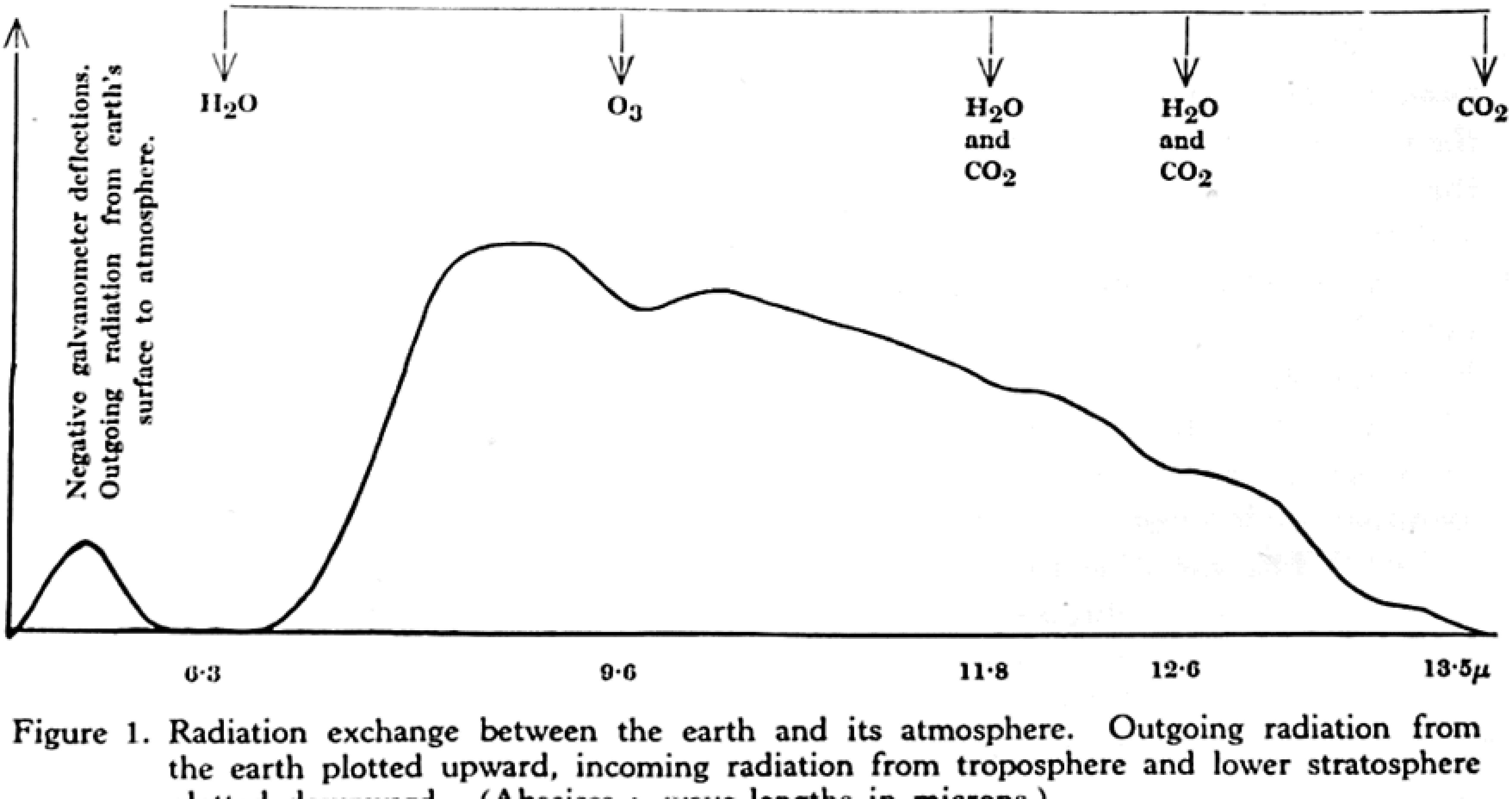

Figure 1. Radiation exchange between the earth and its atmosphere. Outgoing radiation from the earth plotted upward, incoming radiation from troposphere and lower stratosphere plotted downward. (Abscissa : wave-lengths in microns.)

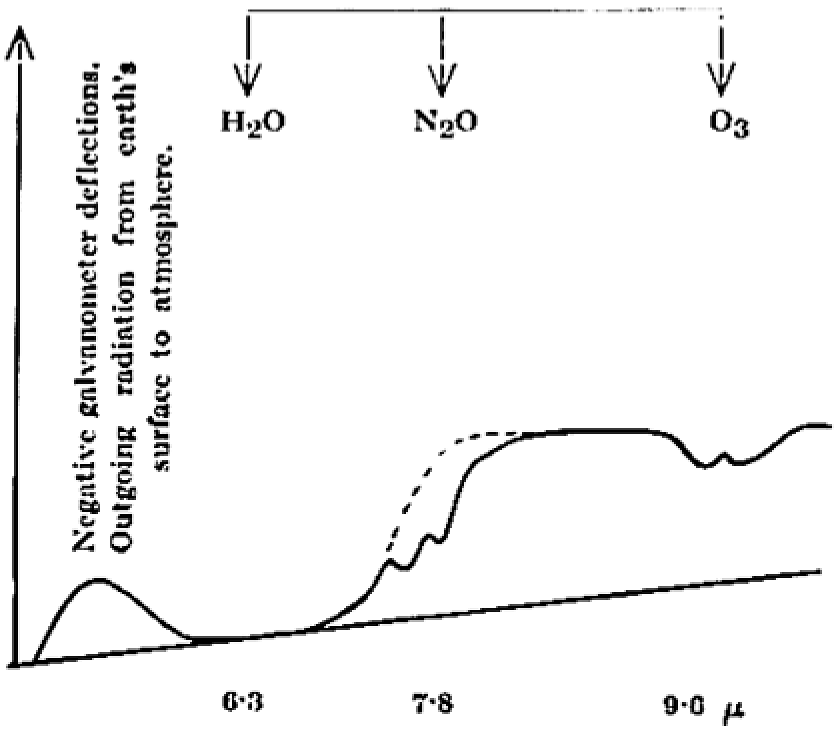

Figure 2. Detail of atmospheric nitrous oxide emission at 7·8 μ and ozone emission at 9·6 μ. Nitrous oxide and ozone emissions plotted downward, superimposed upon outgoing radiation from earth plotted upward. Reconstruction of long wave-length wall of great water band at 6·3 μ shown by broken line.

In these composite emission spectra may be seen the radiations of H_2O, CO_2, O_3, and N_2O. The emission bands of N_2O and CH_4 are blended with the emission of the great water band between 5 and 8μ in Fig 1. Increased resolution serves to delineate the N_2O band in Fig. 2.

3. EFFECTIVE RADIATION TEMPERATURES OF N_2O AND O_3 IN THE TROPOSPHERE AND LOWER STRATOSPHERE

Effective radiation temperatures of the atmosphere may be deduced from observations of the sort described above, provided there is available also a solar or lunar spectrum showing in absorption the bands previously recorded in emission (Adel 1949). The emission and absorption spectra must be secured as closely together in time as possible, with the same instrumentation and dispersion. The emission and absorption spectra must, moreover, refer to the same path through the atmosphere.

In the temperature reduction of the data it is assumed that the molecule in question resides in an isothermal layer. This is a reasonable approximation in the case of ozone, but one of largely unknown value as regards nitrous oxide. In any event, the temperature found may be said to belong to some interior portion of the gaseous layer.

On the assumption of an isothermal layer one may take the emissivity of the ozone or nitrous oxide, at a particular wave length, to be equal to the absorptivity as given by the absorption spectrum. Since the temperature of the thermocouple is known, the curves of emission, such as Figs. 1 and 2, serve to specify the radiation intensities of the ozone and nitrous oxide layers in erg cm^{-2} sec^{-1}/Å, at the particular wave-length in question. The radiation intensity multiplied by the reciprocal of the emissivity is the radiation intensity of an hypothetical black body at the effective radiation temperature of the gaseous layer. The temperature is thus easily obtained from a table of black body radiation intensities.

The sensitivity of the method is good. It may be shown that for both ozone and nitrous oxide,

$$\frac{\Delta T}{T} \simeq \frac{1}{6}\frac{\Delta R}{R}$$

$\Delta T/T$ is the relative error in absolute temperature, which results from a relative error $\Delta R/R$ in the observed radiation intensity of the gaseous layer at the wave length of observation, $7{\cdot}8\mu$ for nitrous oxide, $9{\cdot}6\mu$ for ozone. Furthermore, the atmospheric radiation intensities are sufficiently large to keep $\Delta R/R$ small. R is approximately 1 erg $\mathrm{cm}^{-2}\ \mathrm{sec}^{-1}/\text{Å}$, over the hemisphere, for both ozone and nitrous oxide, and it is believed from repeated observations that $\Delta R/R$ is but a few hundredths. The error in T is thus about 1 per cent or 2°K.

In Table II are listed a few of the ozonosphere temperatures deduced from observations made by the writer at Alamogordo, New Mexico, during the spring of 1948. It is believed that the short-term temperature fluctuations are significant. The values of Table II are in agreement with earlier results obtained by the author at Flagstaff, Arizona (Adel 1947).

TABLE II. DAYTIME OZONOSPHERE TEMPERATURES

Date	Time*	t °C
5 Apr. 1948	1040	—45
	1406	—33
	1420	—36
13 Apr. ,,	1030	—37
	1119	—40
	1136	—40
20 May ,,	0919	—46
	0945	—41
	1006	—38
3 June ,,	1202	—44
	1222	—44
	1242	—43
12 June ,,	1035	—36
	1058	—38
	1123	—38

* Mountain Standard Time

In contrast with the low ozonosphere temperatures, the effective radiation temperature for nitrous oxide is found to lie between 0°C and 10°C. This comparatively high temperature, combined with the well developed distribution of rotational intensities in the atmospheric absorption bands, makes it seem likely that the nitrous oxide resides largely in the troposphere.

REFERENCES

Adel, A.	1947	*Astrophys. J.*, Chicago, **105**, p. 406.
	1949	*The Atmospheres of the Earth and Planets*, by Kuiper *et al.*, Chicago (Univ. Press), chapter X, p. 269.

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The experimental determination of the thermal state of the lower stratosphere

By R. M. GOODY
Solar Physics Observatory, Cambridge University

SUMMARY

Methods are discussed whereby it may be possible to obtain evidence as to the thermal state of the lower stratosphere. Some recent experiments upon the measurement of solar spectra from a high-flying aircraft are described, and some preliminary results upon the water-vapour content of the stratosphere are given. Such data as these should eventually lead to a much better understanding of the rôle of radiation in the lower stratosphere. The possibility of total radiation flux measurements is discussed and it is shown that such measurements would be of great interest but might be rather insensitive. Lastly the possibility of measuring turbulent heat transfer in this region is considered and it is shown that although the direct approach is very difficult, there are several possibilities available for the collection of indirect evidence. It is shown how measurements of the diffusion of particles may be important, how information may accrue from accurate ozone measurements near the tropopause, and finally what information may become available from observations of the behaviour of balloons and aircraft.

1. INTRODUCTION

When dealing with problems of the upper atmosphere it has been a common procedure to assume that the thermodynamical state is considerably simpler than that of the weather-forming layers nearer to the earth's surface. In the problem of explaining the thermal state of the lower stratosphere, it was therefore to be expected that the simplest possible assumption should be pursued until it should prove completely untenable. The assumption made at the beginning of the century by Humphreys (1909) and Gold (1909) was that the tropopause represented the point of transition between a region where convective and turbulent heat transfer predominated (the troposphere), and a region where radiative heat transfer predominated (the stratosphere). This hypothesis has been taken up by many subsequent workers (Emden 1913, Hergesell 1919, Milne 1922, Dobson 1946, Goody 1949), and it has led to an understanding of the division of the lower atmosphere into the two distinctive regions which we call the troposphere and stratosphere, and of the interesting latitude and seasonal variations of the temperature of the lower stratosphere.

Unfortunately, as only too frequently happens in problems related to the earth's atmosphere, direct observation as it becomes available appears to throw doubt upon the validity of this simple and attractive hypothesis. The existence of high wind gradients near the tropopause has been known for some time (Dobson 1920), but recently it has been shown that the tropopause can be the seat of very strong highly localised westerly winds (University of Chicago 1947), which is discouraging if one wishes to assume that turbulence and advection play little part in the thermal equilibrium of the lower stratosphere. Moreover there is direct evidence for the existence of mixing in the lower stratosphere from the measurements upon the concentrations of helium and oxygen, which show that these two gases do not separate out under the action of gravity as would be expected in the absence of turbulence. Even more significant are the German balloon measurements which have demonstrated velocity fluctuations in the lower stratosphere which may be considerably larger than those observed in the troposphere.

In these circumstances one may well wonder why the assumption of a state of radiative equilibrium in the lower stratosphere gives such a reasonable account of the behaviour of the earth's atmosphere. Actually no serious anomaly exists provided that one accepts two simple propositions, namely that the earth's atmosphere possesses some inherent stability and that the thermal structure of the atmosphere is determined by the radiation fluxes which traverse its outermost boundary. It follows that if the sun could be switched off, leaving us in a cold quiescent atmosphere and could be subsequently switched on again, we should eventually find the atmosphere in the same state as it now is. On switching on the sun the atmosphere would initially tend to a state of radiative equilibrium, since apart from molecular transfer, which is unimportant except in the high atmosphere, radiation would be the only means of heat transfer. If this state of radiative equilibrium were stable it would be the state which now exists. Actually such a state will not be hydrostatically stable and will be broken down by the action of advection and convection of heat which it will itself induce, and which radiative effects must maintain. Winds, turbulence, *etc.* are therefore secondary factors, which can only modify the parent state of radiative equilibrium. The only important question therefore is which types of instability can lead to substantial modifications of the state of radiative equilibrium ? If we can allow for these instabilities one at a time, we can reasonably expect to obtain, at each stage, a picture of the earth's atmosphere one step nearer to the observed state of affairs. So far only vertical instabilities have been considered, and already a general account of the atmosphere has emerged. On the other hand the experimental evidence suggests that turbulence and mass movements of air must also be taken into account and therefore we must aim to find out precisely what allowance must be made for these factors.

In the past, attempts to allow for non-radiative heat transfer in the stratosphere have been mainly speculative. Speculation is, however, hardly acceptable in scientific method unless serious difficulties lie in the way of direct observation. In this problem difficulties certainly exist but they are not of such a magnitude that the experimental approach should be abandoned.

There are at least three obvious approaches to the problem :

(1) exact determination of the thermal state of the lower stratosphere under the assumption of radiative equilibrium and comparison with observation,

(2) measurement of the departure from a state of radiative equilibrium by observing the rate of change of total radiation flux with height, and

(3) measurement of the turbulent field near the tropopause in order to deduce the turbulent heat fluxes.

2. MEASUREMENTS OF SOLAR SPECTRA AT HIGH ALTITUDES

The first method is normally a question of measuring spatial distributions of gases which can absorb the short-wave solar radiation or the long-wave terrestrial radiation (notably water vapour, carbon dioxide and ozone), and in the laboratory to determine the absorptive properties of these gases as a function of amount of gas, pressure of the dilutant gas (normally just air) and temperature. This method contains many experimental difficulties, and even given all reasonable information it is not easy to calculate rates of change of the various radiation fluxes in the lower stratosphere.

There is a more direct approach possible to this problem, simply by measuring the absorptive power of the stratosphere in the region of the bands of the most important gases. This requires observation of solar spectra from a spectroscope located near the tropopause. Unfortunately the bands important for the absorption of terrestrial radiation occur in the infra-red region of the spectrum, which is technically the most difficult region to observe. Although serious difficulties therefore arise in work of this nature, the method possesses advantages which make it well worth while. The first advantage is that the experimenter is sure that he is observing relevant information, for if a gas is to be important in the lower stratosphere, it must show absorption bands in the solar spectrum, and conversely if no bands of a gas are observable, then that gas at any rate can scarcely be of any importance as far as thermodynamic considerations are concerned. This will help to resolve much of the doubt which exists as to exactly which gases are of importance in the lower stratosphere. The second advantage is that to a reasonable degree of approximation one does not need to worry about just what factors are responsible for the existence of the band (*i.e.* how the gas is distributed in space, how pressure and temperature affect the area of the band), since for a near-isothermal stratosphere the band area gives, to a first approximation, a measure of the component of the downward flux of radiation for which that particular band is responsible.

Experiments along these lines have been started in Britain (Sutherland and Goody) and in the United States (Strong 1949) although so far progress has been slow and results can only be designated " preliminary." The British experiments have so far been confined to measurements in the region of the 6·3μ water-vapour band, since this band is rather more easy to observe than the more important longer wave bands of ozone, water vapour and carbon dioxide and also because water vapour is of unique interest to meteorologists. A small rapidly recording infra-red spectrometer was used for this work. Wave-length bands of a width of about 1μ were plotted in 7 sec, and the range studied could be anywhere from 3μ to 8·5μ ; the upper limit being set by the fluorite prism which was used. The spectrometer and associated electronics were installed inside the pressure cabin of a Mosquito 32 aircraft, the solar radiation being brought into the cabin by a system of stainless-steel mirrors situated in the air stream. With this installation results could be obtained up to 30,000 ft, which during February 1949, when these results were taken, was unfortunately well below the tropopause. Aircraft oscillations were responsible for difficulties at high altitudes, half-degree oscillations were sufficient to interfere with the recordings (this accounts for some of the irregularities shown on the spectra in Fig. 1), and above 30,000 ft this interference became unmanageable. At the moment modifications are being made to this apparatus in an attempt to include an electronically controlled heliostat which will enable observations to be taken at all heights.

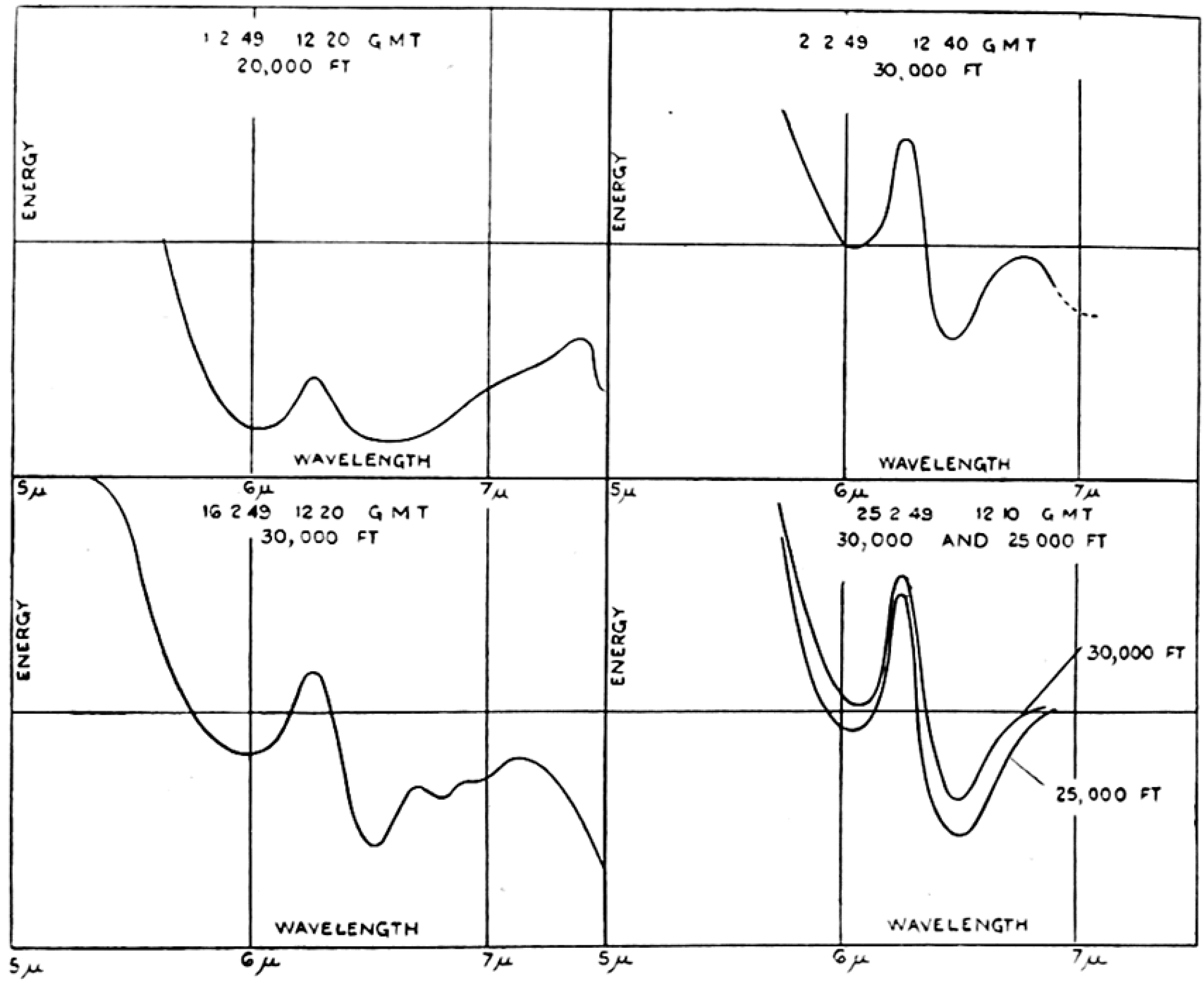

Figure 1. Solar infra-red spectra recorded in the upper atmosphere.

Fig. 1 shows a few spectra which were recorded with this apparatus. It is interesting to see solar radiation penetrating to the spectroscope in a region which so far has been unexplored, owing to the intense absorption of water vapour in spectra recorded at ground level. Although these results are of a preliminary nature a few conclusions can be drawn from the spectra shown. The three ascents to 30,000 ft all give for the quantity of water vapour in a vertical column above this level a figure of about $5 \cdot 10^{-3}$ gm/cm^2, which is equivalent to a condensed layer of water five thousandths of a centimetre thick. This illustrates the sensitivity of the spectroscopic method and the importance of small quantities of water vapour, since absorption in the energetically more important rotation band of water vapour ($20\mu - \infty$) is considerably stronger than that in the $6 \cdot 3\mu$ band.

It is also possible from these figures to make an estimate of the order of magnitude of the water-vapour content of the stratosphere by using radio-sonde and aircraft data to estimate the water-vapour content of the layer between 30,000 ft and the tropopause. For these February days, the water-vapour content of the stratosphere comes out approximately to 10^{-3} gm/cm^2 although the figure is very unreliable since it is the small difference between two larger quantities. We have however every hope that observations can be taken inside the stratosphere and in this case it should be possible to make reliable measurements of this quantity.

On 25.2.49 spectra were taken at 25,000 ft and 30,000 ft, and from these two measurements it was possible to determine the mean frost-point temperature of the layer to be −85 ± 9°F. This estimate was found to embrace the frost points in this layer which had been measured with a visual hygrometer. As a matter of interest the visual hygrometer has, for an inexperienced observer, an error of ±5°F, and therefore when more precise spectroscopic results become available it is possible that humidities in the upper troposphere will be measurable to as great a degree of accuracy as is possible with a visual frost-point hygrometer, although the convenience will be incomparably worse.

Although we have so far only recorded spectra over the range $5{\cdot}3\mu$ to $7{\cdot}7\mu$ it is possible to speculate upon the presence of other gases in the stratosphere likely to affect the thermal state. It can be seen that the reduced absorption by water vapour at 30,000 ft does not reveal any new absorption bands which might be energetically important. Sutherland and Callendar (1942) have compiled a list of polyatomic gases which might be of importance in atmospheric absorption. Our information on the $5{\cdot}3\mu$ to $7{\cdot}7\mu$ region together with considerations as to the likely origin of the gases concerned makes it appear unlikely that any gas except possibly nitrous oxide need be added to the list of those normally considered in the lower stratosphere (*i.e.* water vapour, carbon dioxide and ozone). From other points of view it would of course be extremely interesting to know if any other polyatomic gases exist in the stratosphere, and for this reason it is highly desirable to have spectra, at reasonably high resolution, out to 30μ where the water-vapour rotation band will almost certainly prevent further measurements.

3. TOTAL RADIATION MEASUREMENTS

From the thermodynamical standpoint it would be sufficient to know how the total radiation flux varies with height in the region of the tropopause rather than to know how this variation is built up from the contributions by the different absorption bands of various gases. Under radiative equilibrium the radiation flux, integrated over all angles and wave-lengths, will not vary with height, and therefore measurement of this quantity could show how nearly the lower stratosphere is in equilibrium with the radiation fluxes which traverse it. Long-wave radiation flux measurements have been made with reasonable success near the earth's surface (*e.g.* Robinson 1947) and some measurements have been made up to heights of 5 km in the troposphere by Angström (1928). It is natural therefore that investigators should wish to apply these methods to the stratosphere, and many must have considered the problem (*e.g.* Brewer 1949). There are of course considerable difficulties in this type of measurement and it is therefore worth considering these briefly. Any reasonably simple instrument would have to be of the following general principles. Two thermal detectors, equally black to all relevant wave-lengths would be exposed to the upward and the downward radiation fluxes respectively (the elements could be made unidirectional by silvering one surface, or by some arrangement of mirrors). The elements would reach some equilibrium temperature, and the radiation which they receive could be calculated either from Stefan's Law if the elements were *in vacuo*, or from an auxiliary experiment to determine the emissivity of an element due to all possible causes. With two such elements situated at the temperate tropopause in the absence of solar radiation, a reasonable temperature for the upward-facing element would be 150°K and 250°K for the downward-facing element, provided that they are *in vacuo*. Let us consider how these temperatures will differ when measurements are made at a level 1 km higher. For the purpose of a rough estimate it is probably reasonable to assume that the temperature of the downward-facing element will vary more than that of the upward-facing element. In this case the difference of radiation flux (Δf) at the two levels would be given by

$$\Delta f = 4\sigma \; (250)^3 \; \Delta T,$$

where ΔT is the change of temperature of the downward-facing element over the lowest kilometre of the stratosphere. If the mean temperature of this layer of air is θ, then this change of flux will produce a rate of change of temperature

$$\frac{\partial \theta}{\partial t} = - \frac{\Delta f}{\rho \, c_p \, h}$$

where h is the thickness of the layer (10^5 cm) and the other symbols have their usual meanings. We know that a significant departure from radiative equilibrium would be represented by a rate of change of temperature of 0·1°K/day, or

$$\left| \frac{\partial \theta}{\partial t} \right| \approx 10^{-6} \, {}^{\circ}\mathrm{K/sec.}$$

Substituting values for ρ, c_p and σ gives

$$| \; \Delta T \; | \approx 0{\cdot}1^{\circ}\mathrm{K},$$

as the expected change of temperature of the downward-facing element in the event of a significant departure from a state of radiative equilibrium in the lower stratosphere. It may be fairly safely asserted that this degree of precision of temperature measurement (*i.e.* 0·1 per cent of the temperature difference between the elements) is as yet unattainable in upper-atmosphere research, although temperature changes five times this figure could probably be measured fairly reliably. The method will therefore be rather insensitive although should significant flux changes be measured they will represent a very large error in theories of radiative equilibrium in the lower stratosphere.

Great care would have to be taken in the interpretation of results from this method, for on top of the possible error in temperature measurement, very serious consequences would attend any change in the absorptivity of windows (if used) or the blackness of the receiver, with wavelength of the radiation. Measurements of blackness at long wave-length are normally made with the needs of spectroscopists in mind and are not usually precise enough for thermodynamical calculations, and any decrease in blackness in the range of 20 μ to 50 μ (*i.e.* the range of energetically important wave-lengths covered by the water-vapour rotation band) would have the extremely unfortunate effect of partly nullifying the effect of water vapour.

4. MEASUREMENTS UPON THE TURBULENT FIELD NEAR THE TROPOPAUSE

The state of the theory of turbulent diffusion, even for the accessible layers of the atmosphere near to the ground, may make it appear rather ambitious to suggest that more attention should be paid to the problem of measuring the turbulent field near to the tropopause. Although exact interpretation of such measurements will certainly have to wait, it is not unreasonable to suggest that orders of magnitude might at least be measured, and at the moment it would be a considerable advantage to know even to an order of magnitude the turbulent heat fluxes in the lower stratosphere. From this rather modest viewpoint a number of possibilities suggest themselves.

By observing the distribution of gases in the stratosphere it is possible to gain some idea of the turbulent diffusion necessary to produce such a distribution. The concentration of oxygen has been measured by E. Regener (1936) and the concentration of helium by Glückauf and Paneth (1946), but unfortunately these results, even if significant, provide a rather oversensitive measure of turbulence from the point of view of heat transfer problems. This statement can be illustrated by a very simple order of magnitude calculation originally due to Lindemann (1939) which, although crude, has a clear physical meaning, and manages to avoid the vexed question of the nature of turbulent diffusion.

Consider matter (either a gas or a cloud of particles) mixed intimately with the main oxygen-nitrogen mixture at all heights. In the absence of disturbances the matter will tend towards its Dalton's law distribution and will start to separate out with a mean drift velocity v_D at any given point. If, however at this point, there are disturbing velocities (v_T) of this magnitude, this separation process will be interfered with. If the matter is approximately mixed with the atmosphere we can say

$$v_T > v_D ,$$

while if the matter has more nearly a Dalton's law distribution we may say

$$v_D > v_T .$$

A parcel of dry air moving with a velocity v_T in an isothermal stratosphere will initially change its temperature at a rate

$$\left| \frac{d\theta}{dt} \right| = v_T \, \gamma d ,$$

and therefore by observing the distribution of matter we can obtain limits for the thermal effects caused by the air disturbances. According to Lettau (1947) v_D for helium near the temperate tropopause is 0·5 cm/day. Therefore from the observation that helium is nearly in constant volume proportion in the lower stratosphere we obtain

$$\left| \frac{d\theta}{dt} \right| > 5 \times 10^{-5}\,{}^{\circ}\mathrm{K/day}\ .$$

Since we are interested rather in effects of magnitude 0·1°K/day, helium provides a far too sensitive indicator. It would be much more profitable to work with matter which separates out at about 10^3 cm/day, which corresponds roughly to the rate of fall of a 1μ particle. It is not known whether particles of this size exist in the upper troposphere, but if they do we would gain very important information by observing whether or not they exist also in the lower stratosphere. Condensation nuclei must exist near the tropopause to account for the haze layer which sometimes occurs there. Such nuclei could be detected in the stratosphere by cloud chamber methods, but it is probable that they would be much smaller than 1 μ and hence would not lead to such useful information. Larger particles which might not be condensation nuclei might be detectable from measurements of the light which they can scatter, and a recent analysis of the possibilities of such measurements by Beuttell and Brewer (1949) suggests that this is a hopeful line of investigation.

Ozone is another atmospheric constituent whose distribution can give important information upon turbulent transfer. This gas is almost certainly formed in the stratosphere and subsequently carried downwards by turbulent diffusion (Chapman 1942/3), and therefore it might be supposed that careful measurements upon its concentration near the tropopause might give information upon the diffusive agencies. Two such sets of observations have been made by V. H. Regener (1938) and these show that near the tropopause the ozone concentration increases very rapidly with height. According to Penndorf (1947) this result is understandable if the tropopause represents the transition from a region which is fully turbulent (*i.e.* molecular effects negligible) to a region which is only partly turbulent (*i.e.* molecular effects must be taken into account). The basis for Penndorf's assertion is not clear, since there must necessarily be considerable uncertainty as to the importance of the dissociation of ozone. If however the ozone flux near the tropopause is roughly constant, then the gradient of the ozone concentration gives a direct measure of the diffusion coefficient at this point. By working on the $9{\cdot}6\mu$ ozone band it may prove possible to make measurements of ozone concentration near the tropopause with the spectroscopic equipment which has already been described. If the measurements can be made sufficiently precise it may be possible to check Regener's observations in this way, or if not it should be possible to use a similar installation with an ultra-violet spectroscope measuring the Hartley band of ozone, as has been suggested already by Dobson (1946). It is our intention to make such measurements if it is possible.

Finally let us consider the possibilities of measurements upon turbulent velocities near the tropopause. Just before the war two important sets of observations were made in Germany by Koschmieder (1936) and Junge (1938). Koschmieder made accurate measurements upon the rate of ascent of pilot balloons using two sets of theodolite measurements. His results show that on some occasions there were extremely violent variations of the rate of ascent in the lower stratosphere, even though the ascent through the troposphere had been reasonably steady. Junge also worked with balloons to which were attached small vertical accelerometers. Junge's results agree exactly with Koschmieder, and from a series of five ascents he was led to the conclusion that the turbulence has its maximum intensity somewhere in the lower stratosphere. This is an extraordinary result and is so difficult to fit in with our current ideas that further investigation is required. It would be desirable to make new measurements of these velocity fluctuations and to find out, for example, whether these fluctuations are related to the "bumpiness" which occasionally disturbs high-flying pilots.

The problem of "bumpiness" which has just been mentioned has aroused considerable interest amongst aerodynamicists, and there seems to be some hope that interesting results will accrue from the work which they are doing. Despite the much greater complication of the aerodynamics of aircraft as compared with balloons, it happens that the needs of civil and military flying have led to such a quantity of information about aircraft that it is far easier to interpret exactly the movements of an aircraft than the movements of a balloon. It is also very much easier to measure a number of variables simultaneously from an aircraft than from a balloon, and as a result it is probably more hopeful to look to aircraft measurements rather than balloon measurements if we require quantitative knowledge of the turbulence in the upper atmosphere. Frith (1948) has made measurements upon the temperature and humidity structure of the troposphere from an aircraft, and has succeeded in taking measurements inside what appear to be turbulent eddies with diameters of several miles. If it were possible to measure simultaneously the vertical velocities in these eddies it would be possible to evaluate the flux of heat and water vapour for eddies over a certain size depending upon the lags of the measuring instruments. It is to be hoped that measurements of this nature can be extended to smaller eddies and to altitudes near the tropopause, since the information will be of very great value.

5. CONCLUSIONS

The problem of the thermal state of the lower stratosphere may possibly prove to be the Achilles' heel of the more general atmospheric problem, and if so the difficulties in measurement may be offset by the greater simplicity of the limited system under consideration. It is therefore well worth while trying to make measurements upon this region of the atmosphere, especially since there is good reason to believe that we may already understand the processes which take place at least as well as we understand the processes which take place in the lower atmosphere. High-flying aircraft and balloons which will carry reasonable loads now make systematic measurements possible, and the brief discussion of methods which has been made shows that there are in fact many quite simple measurements which would greatly improve our knowledge. Some of these measurements are in progress and some are projected, and there is therefore every reason to believe that the next few years will show very great steps forward in our understanding of the thermal processes which control the lower stratosphere.

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The calculation of radiative temperature changes

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SUMMARY

The assumptions needed in order to derive simple mathematical expressions for radiative flux and radiative heating are briefly discussed. The mathematical expressions are used to draw general conclusions as to the importance of radiative heating in various conditions. The expressions are transformed into forms suitable for numerical or graphical calculations. A method of determining the temperature distribution in radiative equilibrium is outlined.

1.

At first sight, a quantitative discussion of atmospheric heat radiation is impossible without detailed information about a vast number of different factors. There are three main absorbing gases, and a number of minor ones ; their distribution in the atmosphere needs to be known. The absorptions of each are required at all relevant frequencies ; the variations of absorption with amount of absorbing matter, and with pressure and temperature, need to be known. Then scattering and other perturbing effects have to be taken into account.

Experiment has not yet supplied anything like all the information indicated in this list. Theory can fill some of the gaps, but only if provided with physical data like breadths or strengths of absorption lines, as yet imperfectly known. At present our knowledge of absorption in the whole range beyond 20 μ is regrettably imperfect. At first sight, therefore, one cannot begin to calculate radiative heating and cooling.

As will be realized by those who recall the remarkable success of Sir George Simpson's (1928) calculation of heat balance on the earth, based on inadequate and inaccurate physical data, such an impression is unduly pessimistic. Few of the many uncertainties have any great importance. What needs to be known is the total absorption or emission of radiation, summed over all frequencies, and the possible error at one frequency does not have a large effect on this total. For example, we do not know accurately how the absorption at one frequency varies with amount of absorber : the laws suggested for water vapour by Callendar, Elsasser, Schnaidt and myself indicate that, to lift the smoothed absorption at one frequency from 25 per cent to 75 per cent, the amount of absorber must be increased respectively in the ratios 9, 12, 13 and 18. The resulting uncertainty in the total absorption is, however, slight. Whatever law is used, a given amount of absorber produces practically complete absorption at some wave-lengths, negligible absorption at others, and only in a moderate range of wave-lengths does the difference in law produce any appreciable effects. My own calculations of total absorption give results never differing from Elsasser's by more than two or three per cent of the black body radiation, and even these differences are due partly to different assumed transparencies at the separate wave-lengths (Cowling 1950).

Much the same can be said about the effects of pressure and temperature on the absorption. The pressure effect has not yet been determined with any great precision ; Elsasser's assumption, that a change of pressure in the ratio $1 : k$ is equivalent to changing the quantity of absorber in the ratio $1 : \sqrt{k}$, is convenient but, as theory indicates, usually inaccurate. But pressure corrections to the absorption are unimportant near the ground, where any pressure correction has to be applied only to the small amount of radiation from great heights which is able to penetrate intervening layers. The corrections may be larger at the tropopause, though even here their uncertainties should not be excessive.

Temperature corrections are similar in importance, but more difficult to assess. They arise chiefly from the reduction in absorbing power at the wings of absorption bands at low temperatures, and from the increased importance at such temperatures of long wave-lengths, where the absorbing power is greater. Theory enables us to estimate the temperature corrections with some confidence : but since the theoretical estimates cannot be reconciled with the experimental results of Falckenberg (1939) and Robinson (1950), one cannot feel altogether satisfied with them. One can but hope that their uncertainties will not introduce serious errors : this hope is probably justified, save perhaps in the higher layers of the atmosphere.

Probably it is sufficiently accurate, in view of the other uncertainties, to allow for pressure and temperature corrections by replacing the actual amount of absorber by an equivalent amount at ground pressure and temperature ; the correcting factor may, however, have to be varied according to the problem considered. The advantage of this method is that it reduces the number of variables which have to be taken into account in the later stages of the calculation.

Approximations which reduce the complexity of the mathematical expressions are in any case highly desirable. If simple expressions are used, general properties can easily be identified which would otherwise be lost in a mass of detail. Three simplifying assumptions will be used in what follows, in addition to those made in correcting for pressure and temperature.

(1) The different absorbers are assumed to absorb in non-overlapping wave-length bands.
(2) The fraction of black-body radiation within each of these bands is taken to be the same at all temperatures.
(3) Radiation is supposed to be constrained to travel vertically up or down.

In assumption (3), the slant travel of radiation can to some extent be allowed for by multiplying the quantity of absorber in the way of vertical radiation by a suitable factor. However, the assumption is unsatisfactory to this extent, that it makes it possible only to consider a completely clear or a completely clouded sky.

2.

With these approximations, let $\rho_r(x)$ be the density of the rth absorber at a particular height x above the ground, corrected for pressure, temperature, *etc.*, and let

$$m_r(x) = \int_0^x \rho_r(X)\,dX$$

where X is the vertical coordinate in general.

Let $B(x)$ be the rate of radiation from unit area of a black body at the temperature of air at height x; and let $\phi_r(m_r)$ be the fraction of this which is absorbed by a mass m_r (per unit area) of the absorber. Then when the sky is clear, the flux of energy downward at height x is

$$\sum_r \int_x^\infty B(X)\,\phi_r'(M_r - m_r)\,\rho_r(X)\,dX \qquad . \qquad . \qquad . \qquad (1)$$

where M_r, m_r are used for $m_r(X)$, $m_r(x)$. This expression is a little inconvenient as it stands, because of its dependence on $\phi_r'(M_r - m_r)$, which is less well known than ϕ_r, and is very large near $M_r = m_r$. It can, however, be written in the more convenient form,

$$\sum_r \int B(X)\,d\phi_r(M_r - m_r) \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

which is essentially that used by Robinson and others to get the part of the flux due to water vapour. Robinson effectively replaced $B(X)$ by $B(x)$ in considering CO_2 radiation; as he recognized, this approximation is somewhat questionable near the earth's surface, where the temperature may alter by a few degrees in the lowest metre or so. Either (1) or (2) calls for a numerical integration; a radiation chart is simply a convenient graphical way of effecting the numerical integration. There are similar formulae for a clouded sky, and for the upward radiation.

Radiation charts based on (2) or similar formulae are not always very useful in calculating radiative heating in the atmosphere. The net upward flux of radiation has to be calculated at two neighbouring levels: the difference between the two fluxes, which is appreciable only in special circumstances, gives the total heating between the levels. The mathematical formulae also present difficulties, but in addition to their quantitative uses they also readily indicate the general properties of the heating.

3.

When the sky is clear, the net heating per unit volume is

$$\sum_r \rho_r(x)\left\{-2\phi_r'(0)\,B(x) + \phi_r'(m_r)\,B(0) - \int_0^\infty \phi_r''(|m_r - M_r|)\,B(X)\,\rho_r(X)\,dX\right\} . \qquad (3)$$

The first term in the brackets represents emission of radiation at height x, the second the absorption of radiation direct from the ground at the same height, the third the absorption of radiation emitted by the rest of the atmosphere. [It should be remembered that ϕ_r' is positive, ϕ_r'' negative, for all values of the independent variable.] In the third term, ϕ_r'' is extremely small save when $m_r - M_r$ is small; this represents the fact that most of the radiation absorbed at a given level comes from the immediate vicinity. The short-range radiation has, however, normally only a moderate heating effect; most of the integral when $m_r - M_r$ is small is cancelled by $-2\phi_r'(0)B(x)$. Only when there is an actual discontinuity in the temperature structure in the immediate vicinity does the short-range radiation become important.

Consider first the effect of such discontinuities. Integrate expression (3) by parts ; then it becomes

$$\sum_r \rho_r(x)\{-\phi_r'(\eta_r-m_r)B(\infty)-\int_0^x \phi_r'(m_r-M_r)B'(X)dX \\ +\int_x^\infty \phi_r'(M_r-m_r)B'(X)dX\} \quad . \quad . \quad . \quad . \quad (4)$$

where $\eta_r = m_r(\infty)$. In this expression, the first term in the brackets represents the escape of radiation to outer space ; if a completely opaque absorbing atmosphere, at the temperature of the actual atmosphere at great heights, were introduced above that atmosphere, the extra heating at height x would be

$$\sum_r \rho_r(x)\phi_r'(\eta_r-m_r)B(\infty).$$

A discontinuity in temperature, leading to a discontinuity Δ in B at height ξ, would necessitate adding to (4) a term

$$\pm \sum_r \rho_r(x)\phi_r'(|\mu_r-m_r|)\Delta \quad . \quad . \quad . \quad . \quad (5)$$

where $\mu_r = m_r(\xi)$. Since $\phi_r'(0)$ is large, the discontinuity produces large heating and cooling in its vicinity ; that is, short-range radiation ensures that no ordinary meteorological factors can produce a large permanent discontinuity in a static atmosphere.

For example, Goody (1949) has suggested that, if the tropopause were not at exactly the right height, the escape of radiation to outer space might lead to a discontinuity of temperature at the tropopause. The maximum permissible discontinuity is found by equating the cooling due to escaping radiation with the heating just above the discontinuity, putting $x = \xi$ in (5). This gives

$$\sum_r \rho_r(x)\phi_r'(\eta_r-m_r)B(\infty) = \sum_r \rho_r(x)\phi_r'(0)\Delta.$$

Since $\phi_r'(\eta_r-m_r)$ is, at most, only one-twentieth of $\phi_r'(0)$, this implies that Δ is only one-twentieth of B, and the temperature discontinuity is only one-eightieth of the stratospheric temperature, or 3°C. It is difficult to imagine circumstances which could lead to a greater discontinuity. As Goody shows, any actual discontinuity at the tropopause is unlikely to be appreciable.

The effect of a discontinuity in temperature *gradient* can be seen by taking $B(X)$ as constant above $X = \xi$, say, and B'/ρ_r as constant below ; this approximates crudely to the actual behaviour near the tropopause, so far as water vapour and CO_2 absorptions are concerned. Then, if $x > \xi$, (4) becomes

$$\sum_r \rho_r(x)\{-\phi_r'(\eta_r-m_r)B(\infty)-[\phi_r(m_r)-\phi_r(m_r-\mu_r)](B'/\rho_r)_{X<\xi}\} \quad . \quad (6)$$

where again $\mu_r = m_r(\xi)$. The first term in the bracket again gives the loss in energy to outer space, the second gives the heating due to the discontinuity in temperature gradient. If there is radiative equilibrium just above the tropopause, and no discontinuity in temperature, these two must roughly cancel. It is easy to verify that they are, in fact, similar in order of magnitude ; but a rough calculation indicates that the heating term is the larger if the value of B'/ρ_r used is that just below the tropopause. This is not surprising ; in the actual atmosphere an appreciable part of the heating is contributed by layers well below the tropopause, where B'/ρ_r is smaller in magnitude than near the tropopause.

4.

When there is no discontinuity near the height x, the large contribution to the first integral in (4) from the neighbourhood of $x = X$ is almost exactly cancelled by the similar contribution to the second integral. This difficulty can be avoided by making a further partial integration, but even worse difficulties are thereby introduced at $X = \infty$ unless due care is taken.

Near the ground, an insignificant amount of radiation is received from great heights, and we can chop off the integration in (3) at a finite height ξ (not to be confused with the earlier ξ). Then (4) is replaced by

$$\sum_r \rho_r(x)\Big\{-\phi_r'(\mu_r - m_r)\,B(\xi) - \int_0^x \phi_r'(m_r - M_r)\,B'(X)\,dX \\ + \int_x^\xi \phi_r'(M_r - m_r)\,B'(X)\,dX\Big\}$$

After a further partial integration, this becomes

$$\sum_r \rho_r(x)\Big\{-\phi_r'(\mu_r - m_r)\,B(\xi) + \frac{B'(0)}{\rho_r(0)}\left[\phi_r(\mu_r - m_r) - \phi_r(m_r)\right] \\ + \int_0^\xi \frac{d}{dX}\left(\frac{B'(X)}{\rho_r(X)}\right)\left[\phi_r(\mu_r - m_r) - \phi_r(|m_r - M_r|)\right]dX\Big\} \quad . \qquad (7)$$

The first term in the brackets { } of course represents the escape of radiation to great heights ; even near the ground it cannot be neglected. The second term, like the corresponding term of (6), represents the effect of a discontinuity in B' ; the earth's surface, for radiation purposes, could be replaced by an infinite absorbing atmosphere at the same temperature, and the term represents the discontinuity in B' on entering this equivalent atmosphere. The term produces marked temperature changes near the earth's surface ; in the normal case of a temperature lapse it leads to radiative cooling. The last term in the brackets indicates that changes in B'/ρ_r throughout the whole atmosphere produce comparable effects, though the immediate neighbourhood of $X = x$ is, of course, the most important ; random fluctuations in B'/ρ_r need only be considered if they take place very near the height x. For water vapour and CO_2, B'/ρ_r is negative, and increases in magnitude with increasing height in the troposphere ; hence this term produces cooling in the lower troposphere.

As (7) indicates, very marked changes in ρ_r in a given region will lead to strong radiative effects, just as well as marked changes in B' ; an isolated bank of nearly saturated air would tend to undergo excessive cooling at its upper boundary, and heating at its lower. Well away from the ground, however, radiative heating and cooling are most marked at and near the boundary of a cloud (Möller 1943). The cloud being nearly a black body, so far as infra-red radiation is concerned, its upper surface is rapidly cooled by radiation to outer space in the wave-lengths to which the atmosphere is nearly transparent, while its lower surface is warmed by radiation coming direct from the ground in similar wave-lengths. The atmosphere just outside the cloud may be subject to considerable heating or cooling, in the same way as that near the ground.

5.

The ease with which these qualitative deductions can be made indicates the value of general mathematical formulae. However, quantitative estimates of heating and cooling are also needed. The expression (7) cannot profitably be used for this purpose, because of the awkward behaviour of the integrand when X is large ; it is better to use (4). Remembering (2), one is tempted to write (4) in the form

$$\sum_r \rho_r(x)\Big\{-\phi_r'(\eta_r-m_r)\,B(\infty)-\int_{X<x}\frac{B'(X)}{\rho_r(X)}\,d\phi_r(m_r-M_r) + \int_{X>x}\frac{B'(X)}{\rho_r(X)}\,d\phi_r(M_r-m_r)\Big\} \quad . \quad . \quad . \quad (8)$$

and to use this formula to construct a "radiation-heating chart," in which $B'(X)/\rho_r(X)$ is plotted against $\phi_r(\,|\,m_r - M_r\,|\,)$. This works reasonably well for CO_2, since for this gas $B'(X)/\rho_r(X)$ varies not too greatly in the troposphere, and B' can be taken, without serious inaccuracy, to be zero above an appropriate stratospheric level. For water vapour, however, $B'(X)/\rho_r(X)$ may increase a hundredfold in passing from ground level to the tropopause, because of the rapid decrease in $\rho_r(X)$. For this reason it is best to divide the range of integration in the second integral at a suitable height X' above which there is one-half or one-third of the water vapour above the height x. Then the contribution to (4) from water vapour is written

$$\rho_r(x)\Big\{-\int_{X<x}\frac{B'(X)}{\rho_r(X)}\,d\phi_r(m_r-M_r)+\int_{x<X<X'}\frac{B'(X)}{\rho_r(X)}\,d\phi_r(M_r-m_r) -\phi_r'(\eta_r-m_r)\,B(\infty)+\int_{X>X'}\phi_r'(M_r-m_r)\,dB(X)\Big\} \quad . \quad (9)$$

The third integral in this strictly demands a second radiation-heating chart, but it is frequently possible to approximate to its value without detailed calculations. Ozone demands special treatment, of which a full discussion cannot be given here. However, well below the tropopause it produces negligible heating ; near the tropopause, because of its small emissivity at low temperatures, it is sufficiently accurate to consider only the heating by radiation coming direct from the ground, given by the term $\rho_r(x)\ \phi_r'(m_r)\ B(0)$ of (3).

6.

The temperature of the stratosphere can be calculated on the assumption of radiative equilibrium ; this means that B is assumed known below the tropopause, and is determined above this by the condition that (3) vanishes there. Iteration methods are often used in problems of radiative equilibrium ; in our problem this would mean that, if $B^{(n)}$ is the nth approximation to B, in the stratosphere $B^{(n+1)}$

is found from $B^{(n)}$ by the formula

$$2\sum_{\gamma}\rho_{\gamma}(x)\phi_{\gamma}'(0)\,B^{(n+1)}(x)$$
$$=\sum_{\gamma}\rho_{\gamma}(x)\left\{\phi_{\gamma}'(m_{\gamma})\,B(0)-\int_{0}^{\infty}\phi_{\gamma}''(|m_{\gamma}-M_{\gamma}|)\,B^{(n)}(X)\,\rho_{\gamma}(X)\,dX\right\}\quad . \qquad (10)$$

(in the troposphere, $B^{(n)}$ is understood to be identical with the known value of B). The approximations do not, however, converge at all rapidly ; the dominant influence of short-range radiation ensures that $B^{(n+1)}(X)$ is not very different from $B^{(n)}(X)$ throughout most of the stratosphere. For this reason another method of successive approximation, based on the behaviour of the actual atmosphere, is perhaps preferable. In a static atmosphere, the expression (3) or (4) is proportional to the rate of change of the temperature, $\partial T/\partial t$. Hence, starting with an arbitrary initial temperature distribution which corresponds to $B = B^{(1)}$, a closer approximation to radiative equilibrium can be derived by varying the temperature in the sense indicated by $\partial T/\partial t$; this closer approximation gives $B = B^{(2)}$. Using this, a further approximation $B = B^{(3)}$ can be calculated in much the same way ; and so on.

ADDENDUM

During the Oxford meetings, I was told by Dr. Godson of a paper by A. Bruinenberg (1946) on the calculation of radiative temperature changes, which appeared to have much in common with the ideas set out above. I have now had an opportunity of reading the paper ; the resemblance with some of my ideas is very marked.

Bruinenberg's treatment differs from my own as follows.

(i) He does not restrict himself to vertical radiation ; this introduces no essential extra complication.

(ii) He replaces the product $B(X)\,\phi_{\gamma}(m_{\gamma})$ by a single function of two variables, $S_{\gamma}(m_{\gamma}, T)$. This enables him to take into account the varying importance of the different wave-lengths at different temperatures, and to that extent makes for greater accuracy. On the other hand, it leads to greater difficulties in manipulating the mathematical expressions, and obscures certain general results — *e.g.*, the essential equivalence, in their effects on the heating, of changes in ρ_{γ} and in $B'(X)$.

(iii) He does not attempt to give a radiation-heating chart, which would be difficult to construct in terms of his function S_{γ}, but employs numerical integration alone.

His treatment should be of interest to all concerned with the accurate determination of radiative temperature changes.

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551.521 : 551.524.4 : 551.524.7

Two notes on temperature changes in the troposphere due to radiation

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SUMMARY

1. The radiative temperature of short grass in cloudless conditions differs substantially from the temperature of the air near the surface. This makes possible considerable radiative exchange. Three specific cases are examined, and the radiative flux changes between the surface and 50 cm investigated using the Kew chart. In one case (a calm night) there is an approach to radiative equilibrium in this layer.

2. Radiative flux changes between the surface and 500 mb are computed for a cloudless June day, using the Elsasser chart. The resulting heat loss, summed over the 24 hours is shown to be approximately compensated by the heat gain due to convection at the surface. Radiative heating during the afternoon extends from the surface up to about 850 mb.

1. RADIATIVE HEAT EXCHANGE NEAR A SHORT GRASS SURFACE

(a) *Temperature and humidity data*

In a previous paper (Robinson 1950) a simple method of measurement of the radiative temperature of short grass was described. This has been modified to allow continuous recording and some records have been obtained at Kew Observatory. That for 21 June 1949, a cloudless day, is shown in Fig. 1. As far as can be seen from three months' results it is not an extreme case. These large temperature differences introduce the possibility of large changes in radiative flux, which may be computed by radiation-chart methods if details of temperature and humidity are known. Mean values over half-hour periods have been measured at Kew on a number of occasions in collaboration with Mr. N. E. Rider, and three examples are shown in Fig. 2 — midday and early night with moderate winds and cloudless sky in June, and early night with very light winds and cloudless sky in November.

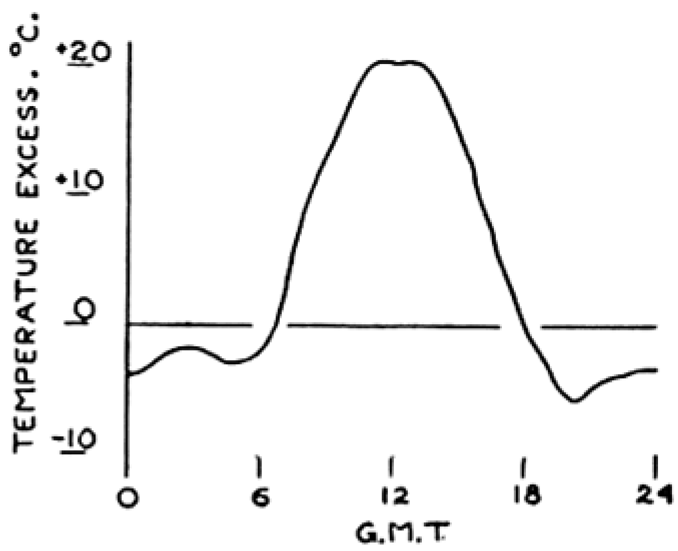

Figure 1. Record of the temperature difference between the surface of short grass and the screen at Kew Observatory 21 June 1949.

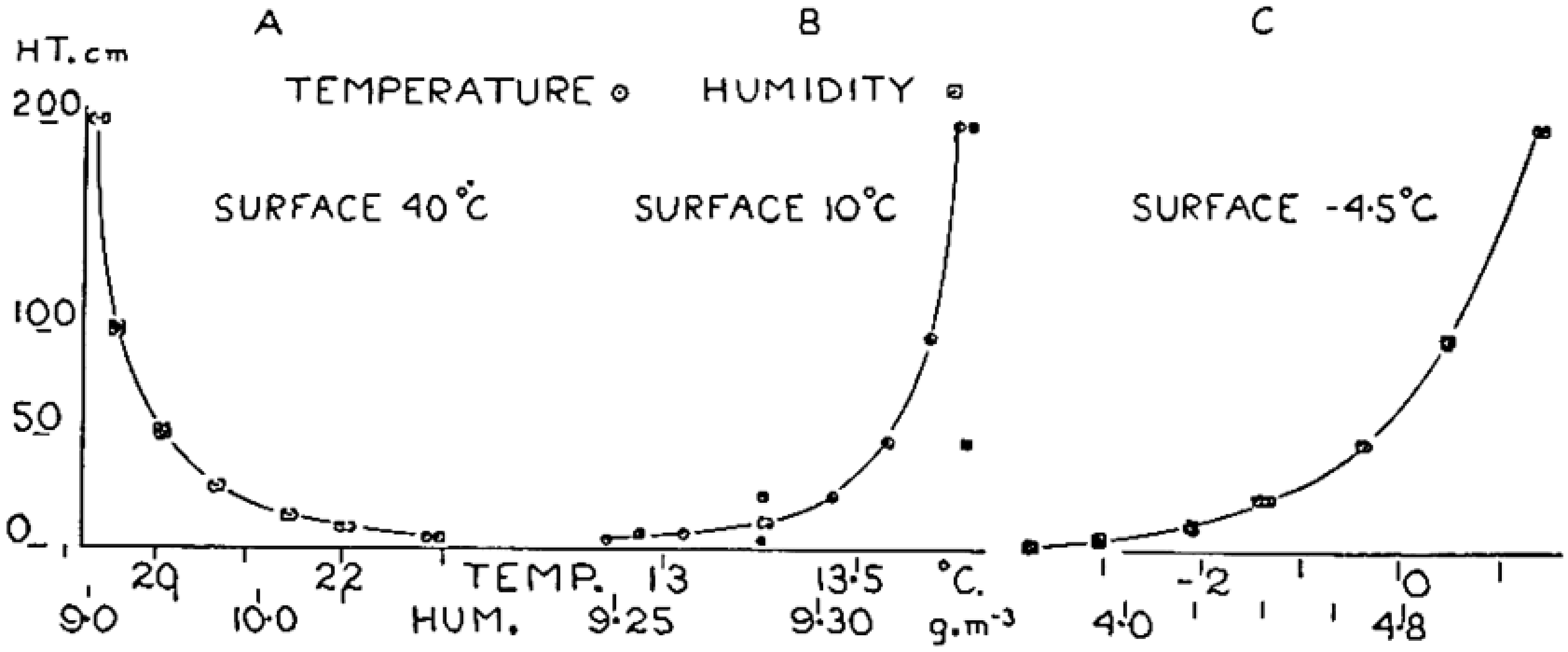

Figure 2. Examples of the temperature and humidity structure in the lowest layers of air over short grass at Kew Observatory: A—21 June 1949, 1100 G.M.T., B—7 June 1949, 2130 G.M.T., C—26 November 1948, 1745 G.M.T.

(b) *Computation of the radiative flux*

This was carried out, using the Kew chart, for layers approximately 25 cm thick. There were no homogeneous temperature and humidity data above 2 m, but it was found that the computations in the lowest 50 cm were insensitive to any reasonable variation in these values.

21 *June* 1949, 1100 *G.M.T.*

Net outward long-wave radiative flux at the surface (measured) $\approx 5{\cdot}2 \times 10^{-3}$ cal. cm^{-2} sec^{-1}.

Change in radiative flux 0-50 cm (computed) $\approx -1{\cdot}7 \times 10^{-4}$ cal. cm^{-2} sec^{-1}.

Corresponding rate of change of temperature $\approx +1 \times 10^{-2}$ °C sec^{-1}.

Observed rate of change of temperature $\approx +3 \times 10^{-4}$ °C sec^{-1}.

7 *June* 1949, 2130 *G.M.T.*

Net outward long-wave radiative flux at the surface (measured) $\approx 1{\cdot}6 \times 10^{-3}$ cal. cm^{-2} sec^{-1}.

Change in radiative flux 0-50 cm (computed) $\approx +2{\cdot}0 \times 10^{-5}$ cal. cm^{-2} sec^{-1}.

Corresponding rate of change of temperature $\approx -1{\cdot}5 \times 10^{-3}$ °C sec^{-1}.

Observed rate of change of temperature $\approx -3 \times 10^{-4}$ °C sec^{-1}.

26 *November* 1948, 1745 *G.M.T.*

Net outward long-wave radiative flux at the surface (measured)· $\approx 1{\cdot}0 \times 10^{-3}$ cal. cm^{-2} sec^{-1}.

Computed change in radiative flux 0-50 cm $\approx 0{\cdot}0$ ($< 1 \times 10^{-6}$ cal. cm^{-2} sec^{-1}).

Corresponding rate of change of temperature $\approx 0{\cdot}0$.

Observed rate of change of temperature $\approx -1 \times 10^{-3}$ °C sec^{-1}.

(c) *Discussion*

The first two cases, with moderate winds (about 300 cm sec^{-1} at 1 m), show a large tendency to radiative heating or cooling which is almost balanced by convection. Robinson (1950) has obtained some support for similar radiation-chart computations by direct measurement of the change in radiative flux near the surface at night. It is of interest to note that these changes are greater by a factor of 10^3 to 10^4 than those considered by Goody (1950) to be potentially significant in the stratosphere.

The third example appears to show an approach to radiative equilibrium in the layer from the surface to 50 cm. It is one of four instances in which this has so far been observed at Kew. In all cases the mean wind at 1 m, over a 30-minute period, was less than 50 cm sec^{-1}. The present example was observed immediately before the formation of a thick ground fog with an exceptionally well-marked upper surface.

It appears from these examples that in cloudless conditions radiative transfer plays a part of importance equal to that of convection (turbulence) in the transfer of heat from the ground and the diurnal variation of temperature.

2. AN EXAMPLE OF RADIATIVE AND CONVECTIVE HEAT CHANGES IN A CLOUDLESS AIR MASS

(a) *Introduction*

The purpose of this note is to give an example of the application of the Elsasser radiation chart, possibly the best known and most generally useful of the available methods of computing temperature changes due to radiation, to a problem of interest to the synoptic meteorologist. Sutcliffe (1949) has referred to the problem of the non-adiabatic temperature changes in an anticyclone ; we will consider a somewhat idealised version of the situation of 21 June 1949 over SE. England, taking the Larkhill upper air soundings and the Kew surface temperatures as representative, and confining attention to the layer between the surface and 500 mb. (The uncertainty of radiation chart computations in the upper troposphere has been mentioned by Robinson (1950), and it is usual to consider the humidities measured by radiosondes as at least equally suspect.)

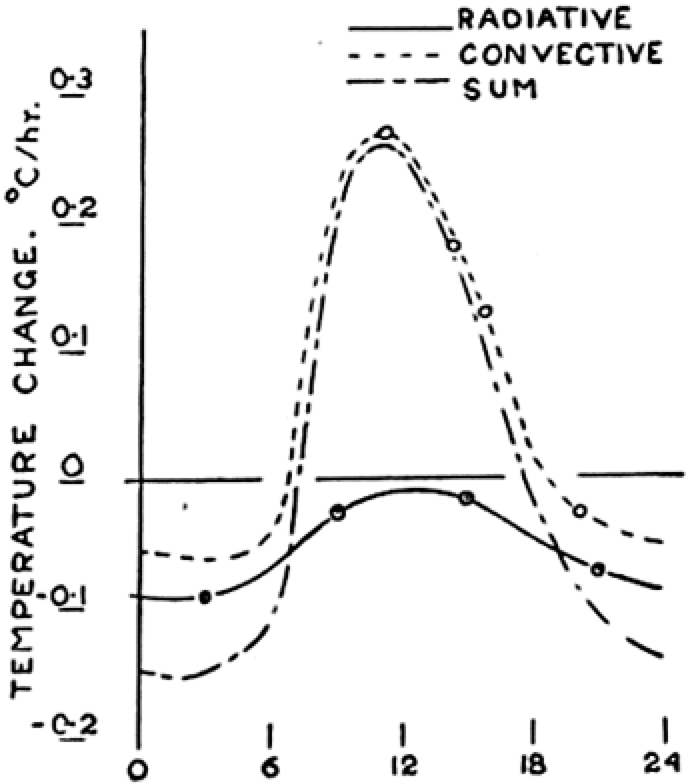

Figure 3. Mean temperature changes in the layer between the surface and 500 mb, 21 June 1949. (Data from Kew and Larkhill)

(b) *The radiative temperature changes*

The surface temperatures are given in Fig. 1. Using these and the four Larkhill ascents the rates of temperature change due to radiation computed for the whole layer from the surface to 500 mb, and expressed as a mean for the whole layer, are shown in Fig. 3, where the full line shows interpolated values. Layer-by-layer computation for the 1500 G.M.T. ascent indicates that the radiative heating, shown in the previous note to be present close to the ground, decreases rapidly at first with height, but does not change to the radiative cooling (often spoken of as " universal ") until about the 850-mb level.

(c) *Temperature changes due to convective heat transfer from the ground*

To complete the non-adiabatic heating term for the layer considered it is necessary to know the heat gained by convection at the surface. Certain measurements of this term were made at Kew on 21 June and the preceding and following days when conditions were very similar. They are plotted in Fig. 3, the broken line giving interpolated values obtained with the help of an empirical formula involving wind speed and surface temperature. The resultant mean rate of temperature change due to non-adiabatic processes, the sum of radiative and convective effects, is also shown in Fig. 3. Integration of these curves gives the following results.

Mean temperature change in layer in 24 hours due to radiation $\approx -1{\cdot}3$ °C.
Mean temperature change in layer in 24 hours due to convection $\approx +1{\cdot}3_7$ °C.
Net mean temperature change from surface to 500 mb in 24 hours $\approx +0{\cdot}0_7$ °C.

(d) *Discussion*

In the case chosen radiative cooling and convective heating of the layer considered as a whole are almost balanced, and the conditions are sufficiently typical to suggest that this is approximately true in general for cloudless summer anticyclones in Southern England. (It is assumed that the Kew surface temperatures and convective flux results are representative.) The presence of cloud would be expected to change the balance completely by introducing the possibility of realisation of latent heat in the layer, by reducing mean surface temperature, and, probably most important, by introducing radiating surfaces which greatly increase the radiative heat loss.

ACKNOWLEDGEMENTS

Much of the material used in these notes was obtained in the course of joint work with Mr. N. E. Rider, of which we hope to publish a complete account. I also wish to thank Mr. R. H. Eldridge, who made the radiation computations used in the second note. Thanks are due to the Director of the Meteorological Office for his permission to publish the notes.

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Discussion

Dr. T. W. Wormell (*Cambridge University*) : Professor Adel opened his remarks with a description of his own pioneer work in the infra-red solar spectrum. This work yielded strong evidence for the existence of nitrous oxide in the earth's atmosphere. An investigation was undertaken at Cambridge, England, by Shaw, Sutherland and myself, (*Phys. Rev.*, **74**, p. 978), using a high resolution prism spectrograph with the double object of making this identification conclusive, and of finding whether the presence of nitrous oxide in the atmosphere was a widespread phenomenon. It proved impossible, working from near sea-level, to observe either of the bands at 7·8 μ and 8·6 μ which were found by Adel. The former is lost in the water band, centred at 6·3 μ, and the latter, a weak band, is concealed by other structures in the spectrum. Observations at shorter wave-lengths, using a prism of lithium fluoride, led however to the definite identification of three bands in the solar spectrum at 3·90 μ, 4·06 μ and 4·50 μ as being due to nitrous oxide. It is thus established that nitrous oxide is a widespread and normal constituent of the atmosphere, the total amount being of the same order as the total amount of ozone.

A recent report describing work done under the auspices of the Admiralty, *Atmospheric transmission in the* 1—14 *micron region*. A.R.L./R.4/E.600 (1949), indicates that radiation, after traversing a horizontal path of 2 mi near sea-level, shows the nitrous oxide bands at 3·9 μ and 4·5 μ with considerable intensity. This affords direct confirmation of Adel's conclusion, from his estimate of mean temperature, that most of the nitrous oxide, unlike the ozone, occurs in the lowest layers of the atmosphere.

Mr. R. S. Scorer (*Imperial College*) : Large-scale weather systems give rise to vertical velocities in the troposphere of the order of 1 km/day. The consequent vertical velocities in the stratosphere may be about one-tenth of this, but even then would transport heat and radiating constituents of the atmosphere about ten times as fast as in the greatest turbulence. The large vertical velocities of the tropopause, of the order of 100 mb/day in vigorous weather systems, often lead to a new tropopause being formed, and this process is far more important in disturbing radiative equilibrium than any effects of turbulence.

Dr. G. M. B. Dobson : I would like to direct attention to the observation reported by Professor Adel that a much larger amount of the sun's infra-red radiation is scattered by the clear atmosphere than is the case in the visible region. On theoretical grounds one would expect the scattering to be smaller for infra-red radiation than for visible light ; indeed the use of infra-red filters is well known when objects at great distances are to be photographed. I understand that Professor Adel has no explanation to offer for this increased scattering and I would like to know whether the same effect has been found by Dr. Wormell in his work at Cambridge. It would be interesting if any member of the Conference can suggest a reason for this effect*.

In view of its importance in the theory of the thermal balance in the stratosphere I would like to ask if there are any results yet available from the work conducted by Dr. Strong on the effect of pressure and temperature on the absorption coefficients of water vapour and ozone.

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* See remarks by Prof. Prins on p. 33. *Ed.*

I notice that the infra-red spectra of the sun's radiation shown by Dr. Wormell indicate that the absorption by ozone in the atmosphere near 9·5 μ is nearly complete whereas the American work usually indicates an absorption of about 50 per cent. Could Dr. Wormell tell us whether the ozone content of the atmosphere was likely to be very high on the days when his spectra were taken or was the sun rather low ?

Mr. J. M. Craddock (*Meteorological Office*) : The processes of atmospheric radiation are evidently very complex. In problems such as the forecasting of fog or of night minimum temperatures, the forecaster wants to know the *total* rate of radiative heat loss at the earth's surface in terms of a few easily measured parameters. The Kew radiation chart can do this to within 10—15 per cent. Is there any method capable of quick application and using limited data which can reach a similar level of accuracy ?

Mr. J. S. Sawyer (*Meteorological Office*) : An examination of synoptic charts for levels in the upper atmosphere confirms the values of vertical velocities in the lower stratosphere suggested by Mr. Scorer as associated with weather systems such as depressions. Vertical air movements of 1 km in 12 hr seem to occur, but in many cases downward motion is followed within 24 hr by upward movement of the same air. Under these circumstances may we expect an approximate radiation balance over a period ? However, there are occasions when synoptic analysis suggests more permanent changes in air level near the tropopause and such occasions would disturb the radiation balance.

Dr. V. J. Schaefer (*G.E.C., U.S.A.*) : I would like to raise the question of the possibility that the presence of ozone and nitrous-oxide absorption on bands might be partly due to the presence of such materials in the lower atmosphere due to industrial pollution. Observations by Stanford Research Institute in the Los Angeles " smog " showed high levels of ozone-like materials, *ca.* 1 part per million or more during periods of high smog levels. Nitrous oxides produced by air pollutants might modify the results using horizontal searchlight beams mentioned by Dr. Wormell.

Prof. P. A. Sheppard : Could Professor Adel tell us whether the observed scatter of O_3 radiative temperatures on a given day is significant and, further, whether there is any evidence for an appreciable diurnal variation in temperature in the effective layer of emission.

Mr. A. W. Brewer (*Clarendon Laboratory*) : In discussing radiation in the atmosphere it is important that the interesting physical problems involved should not make the meteorologist forget his main object. This is the study of complex and very varied consequences of the operation of the atmosphere as a thermodynamic machine by which the kinetic energy of the winds is produced.

Now it is well known that by the Second Law of Thermodynamics a heat engine must absorb heat from a source at high temperature and discharge heat into a sink at low temperature ; but if the properties of the working substance are defined the Second Law can enable us to make further deductions. It is familiar knowledge that in a steam engine, working with saturated steam, the boiler must be at high pressure, and the condenser must be at low pressure, and the relation between the temperatures and pressures is given by the vapour-pressure curve for water ; the differential form of which is the Clausius-Clapeyron equation.

Similarly it can be shown that for a reversible machine using a perfect gas, at the source of heat the gas must be at a high pressure and at the sink the pressure must be lower, such that by adiabatic expansion the gas may be cooled from the temperature of the source to the temperature of the sink. Thus in the atmosphere where substantial pressure differences only exist vertically, the sink of heat must be in the upper air. If there exists a sink of heat at the ground this is equivalent to cooling the boiler of a steam engine and retards the action of the atmosphere, but cold ground does of course also assist in radiative cooling of the upper air and in this way it plays a positive part in maintaining the movements of air which cause the weather.

Thus by thermodynamic reasonings we can show that to produce the mechanical energy which we see being continually developed in the atmosphere there must be a significant radiative sink of heat in the upper air. When we make allowance for the haphazard and inefficient operation of the atmosphere we find that there must be radiative cooling throughout substantial depths of the upper troposphere corresponding to a rate of cooling of the air by about 1°C/day. For a small volume of gas we have the relation $\operatorname{div} R = \rho\, C_p \dfrac{d\theta}{dt}$ where R is the flux of radiant energy. It is this value of the divergence of the radiative flux which the meteorologist will require to know.

DR. E. T. EADY (*Imperial College*) : Computations of three-dimensional trajectories in the free atmosphere, made by Mr. Howells, show an average rate of decrease of potential temperature (in cases where the air remains unsaturated and turbulence might reasonably be expected to be relative slight) of about 1°C/day, thereby confirming at least the order of magnitude of the results of radiation calculations.

MR. E. GOLD : In reference to Mr. Brewer's demands for a "sink" cooling at the rate of 1°C per day I should like to say that I computed, with the data available 40 years ago, the rate at which the atmosphere between the surface and 250 mb would cool by radiation (in the absence of solar radiation) and found it to be 1°C per day — the exact agreement is no doubt an accidental coincidence. But it does indicate that radiation could provide the " sink " necessary for the working of the engine.

Does Professor Cowling's statement that an enormous amount of atmosphere is necessary to obtain nearly complete absorption refer to a specially selected wavelength. The absorption, by a length z, of atmosphere of radiation of wavelength λ is $I_\lambda\, d\lambda\,(1-e^{-x})$ where $x = \int_0^z b\, dz$ and I was under the impression that for the wavelengths for which water vapour absorbs the value of b and therefore of x is usually so large that a comparatively shallow layer of the troposphere is enough to give practically complete absorption.

Mr. Goody suggests that strong winds in the lower stratosphere and near the tropopause present a difficulty in accepting the general radiation-equilibrium explanation of the stratosphere. I think these winds are a necessary consequence of the thermal structure of the troposphere : the division between troposphere and stratosphere is not a smooth spherical surface but is subject to distortion, both limited, as when a thundercloud thrusts upwards, and extensive as over a frontal system. But the broad scheme of an atmosphere in radiative equilibrium, transformed in the lower levels into a convective troposphere through solar heating of the earth's surface remains, I think, a true representation.

DR. J. A. PRINS (*Delft*) : Dr. Adel's strong scattering inside the infra-red absorption bands when the sky close to the sun is observed looks more or less like resonance radiation. The scattering close to the sun, but only for visible wavelengths, has also been measured by Dr. Reesinck at Wageningen (1946, *Meded. Landb-Hoogesh.*, Wageningen, **48**, Verh. 3).

PROF. A. ADEL (*in reply*) : In reply to Dr. Schaefer. It may be of interest in this connection to call attention to an hypothesis regarding the origin of nitrous oxide, which was put forward in 1946,* and which maintains that much, if not all, of the atmospheric nitrous oxide may come from escaping soil air.

In reply to Prof. Sheppard. The observed scatter of ozone radiative temperatures appears to be significant. On the days of variations in ozonosphere temperature, the apparatus, by laboratory test, appears to be as stable as on the days of little or no variation.

Observations in the heat spectrum of the moon reveal a night cooling in the effective layer of ozone emission of only a degree or two, centigrade.

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* A. Adel, 1946, A Possible Source of Atmospheric Nitrous Oxide, *Science*, **103**, p. 280.

CENTENARY SYMPOSIUM

ON

PHYSICS OF CLOUDS AND PRECIPITATION

Held at Oxford, the morning of 29 March 1950

Chairman : Dr. G. M. B. DOBSON, F.R.S.

551.574.1

Introductory Address by Chairman.

There has recently been a wave of renewed interest in the physics of clouds and particularly in the details of the initiation of the growth of ice particles in a cloud. This is largely due to the theories of the formation of rain put forward by Bergeron (1935) and by Findeisen (1938). While many years ago there was much discussion of the way in which cloud droplets grow to the much larger raindrops, little progress was made, and even Sir Napier Shaw, who did so much to bring physics in to meteorology, could write in his *Manual* :— " When the condensation of vapour is continued beyond the limit of the carrying power of cloud, rain is formed . . . ." The theories put forward by Bergeron and by Findeisen are founded on the fact that while waterdrops can continue to exist as liquid drops much below 0°C the vapour pressure over a waterdrop is appreciably greater than the vapour pressure over an ice particle at the same temperature. The difference in vapour pressure increases as the temperature falls below 0°C until it reaches a maximum at about —12°C after which it falls slowly with further fall of temperature. It is known that there are very few nuclei in the atmosphere on which ice can form directly by deposition from the vapour except at very low temperatures ; hence even at temperatures well below 0°C waterdrops, and not ice particles, are first formed when air is cooled below its dew-point. If, in such a supercooled water cloud, there are a few ice particles they will be in a supersaturated atmosphere and will consequently grow rapidly by direct deposition of water vapour. The theories of Bergeron and of Findeisen suggest that, except in the case of drizzle or very light rain, all raindrops start as ice crystals, and without the presence of ice crystals a cloud cannot give heavy rain.

The study of ice-forming nuclei in the atmosphere and the processes by which ice particles can be formed in a cloud becomes, therefore, of great importance in meteorology. The experiments are well known in which supercooled water clouds have been turned into ice clouds by seeding them with minute ice particles or ice-forming nuclei so that there is definite evidence that the conversion of a water cloud into an ice cloud can take place in this way. Moreover the experiments indicate that, while the seeding is done in a narrow band, the clearing in the clouds spreads sideways until it is two or three miles wide. There must therefore, be some mechanism by which, if ice particles are once introduced into a cloud, they can produce other secondary ice particles which in their turn again produce others.

Recent work in the laboratory has greatly increased our knowledge of the temperatures at which water droplets usually freeze and also of the properties of the various ice-forming nuclei which can initiate the direct deposition of ice from the vapour in supersaturated conditions, (see papers by Ludlam (1948) and Mason (1950) for excellent summaries of recent work). With regard to the freezing of water droplets, Haverly (1949) has shown that small raindrops, say 1 mm in diameter, usually freeze at a temperature of about —15°C, while with smaller drops, such as are usually found in clouds, the freezing point depends on the size, and falls rapidly from about —15°C for drops of diameter 0.4 mm to about —30°C for drops of about 0·05 mm diameter, with still lower freezing points for smaller drops.

With regard to the atmospheric nuclei on which ice can form directly in supersaturated air, we now know that, as the temperature falls, more and more nuclei become active and each nucleus seems to have a definite threshold temperature below which, and only below which, it will act as an ice-forming nucleus. Above about —15°C there are probably no active ice-forming nuclei present in sufficient numbers to be of importance in the atmosphere. Between —15°C and —32°C, the number of active ice-forming nuclei slowly increases as the temperature falls, while at about —32°C a number of nuclei which are found in the lower atmosphere become active. The temperature of —32°C is therefore likely to be an important critical temperature in meteorology though it is doubtful whether there are many of these " —32°C nuclei " in the upper atmosphere except where they have been carried up in strong rising currents such as in cumulonimbus clouds. The temperature of —41°C is another important critical temperature as all particulate matter present in the atmosphere probably becomes active as ice-forming nuclei at this temperature. An interesting point about all these ice-forming nuclei to which we have been referring is that ice does not form on them — even below their threshold temperatures — when the air is slightly supersaturated with respect to *ice*, but only after supersaturation with respect to *water* is reached or at least very closely approached, *i.e.* considerable supersaturation with respect to ice.

As a result of this, our newly acquired laboratory knowledge, we should expect supercooled water clouds to be common in the atmosphere at temperatures down to at least —15°C while we should expect ice clouds to be formed whenever the temperature is below —41°C. What happens in the intermediate temperatures is much less clear ; are the ice particles found in clouds at these temperatures formed by the freezing of water droplets or by the deposition of the vapour directly as ice crystals ? We now know that once an ice particle is formed it can form more sublimation nuclei by throwing off minute splinters of ice either when vapour condenses on it or — and much more frequently — when water freezes on it. This 'splintering' process seems likely to be responsible for the sort of ' chain reaction ' by which, once some ice-forming nuclei are introduced into a cloud, they can give rise to other ice-forming nuclei and the process continue indefinitely. It is possible that a very few ice-forming nuclei may be all that is required to transform a supercooled water cloud into an ice cloud and the very scarcity of such nuclei makes their study very difficult.

I expect that many of the speakers who follow will be reporting the results of important laboratory studies on atmospheric nuclei and the freezing of water droplets ; I hope however that we shall not neglect the study of the clouds themselves and that as a result of this discussion we shall be able to arrive at some better understanding of the processes which actually take place in the clouds.

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Propagation of visible and infra-red radiations through fog

By M. J. BRICARD
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The study presented here is only a starting point for researches under way, concerning the transparency of natural fog.

Let us consider a cylindrical monochromatic beam, proceeding through an atmospheric layer of thickness x, and an element of path dx along the incident rays. Let ϕ be the flux striking the element of thickness dx. A part of ϕ is diffused. We shall assume that this part is proportional to ϕ, and write $d\phi = - K\phi \, dx$, K being the coefficient of apparent absorption, in contrast to selective absorption, or true absorption, which will not be considered here. By integrating for the thickness x,

$$\phi = \phi_0 \, e^{-Kx} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

ϕ_0 being the original flux, and ϕ the transmitted flux ; we shall call the ratio $T = \phi/\phi_0$ the transmission factor of the medium.

The diffusing medium that we have to consider here consists of small water drops in suspension in the air, the dimensions of these drops being of the order of the wavelength of light or larger, and we suppose that the molecular diffusion by the air is very small compared to that of the droplets. The ratio of total flux diffused from the incident wave by a transparent, spherical, dielectric particle of refractive index $n = 4/3$ has been calculated by Stratton and Houghton (1931) according to Mie's theory (1908), and is equal to $A = 2\pi R^2 f(R/\lambda)$ where

R = Particle radius
λ = Wave length of the light.

The function $f(R/\lambda)$ is represented in Fig. 1, the abscissa being $\alpha = 2\pi R/\lambda$. This shows two maxima at about $\alpha = 6$ and $\alpha = 15$, separated by a minimum at $\alpha = 12$; it approaches unity and then stays constant for values α greater than 30.

If the radius is large enough, we see that the flux intercepted by the sphere is twice the flux intercepted by its section. The coefficient defined by the relation (1) is given by :

$$K = 2N\pi R^2 f\left(\frac{R}{\lambda}\right) \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

where N = number of fog droplets contained in unit volume.

In fact, all drops have not the same dimensions. The distribution of the particles according to their radius follows experimental relations depending on the constitution of the medium, and this is important to know. For this, we will consider a sufficiently large number N_0 of fog particles so that statistical laws may be employed. We shall determine the number Δn of particles whose radius is between $R - \Delta R$ and $R + \Delta R$, ΔR being a small variation on R that we can obtain experimentally with reasonable certainty, and the curves representing the ratio $\Delta n/\Delta R$ bave been deduced as a function of R. At b in Fig. 2 is an example of a distribution curve characterizing the fog employed during the first series of measurements

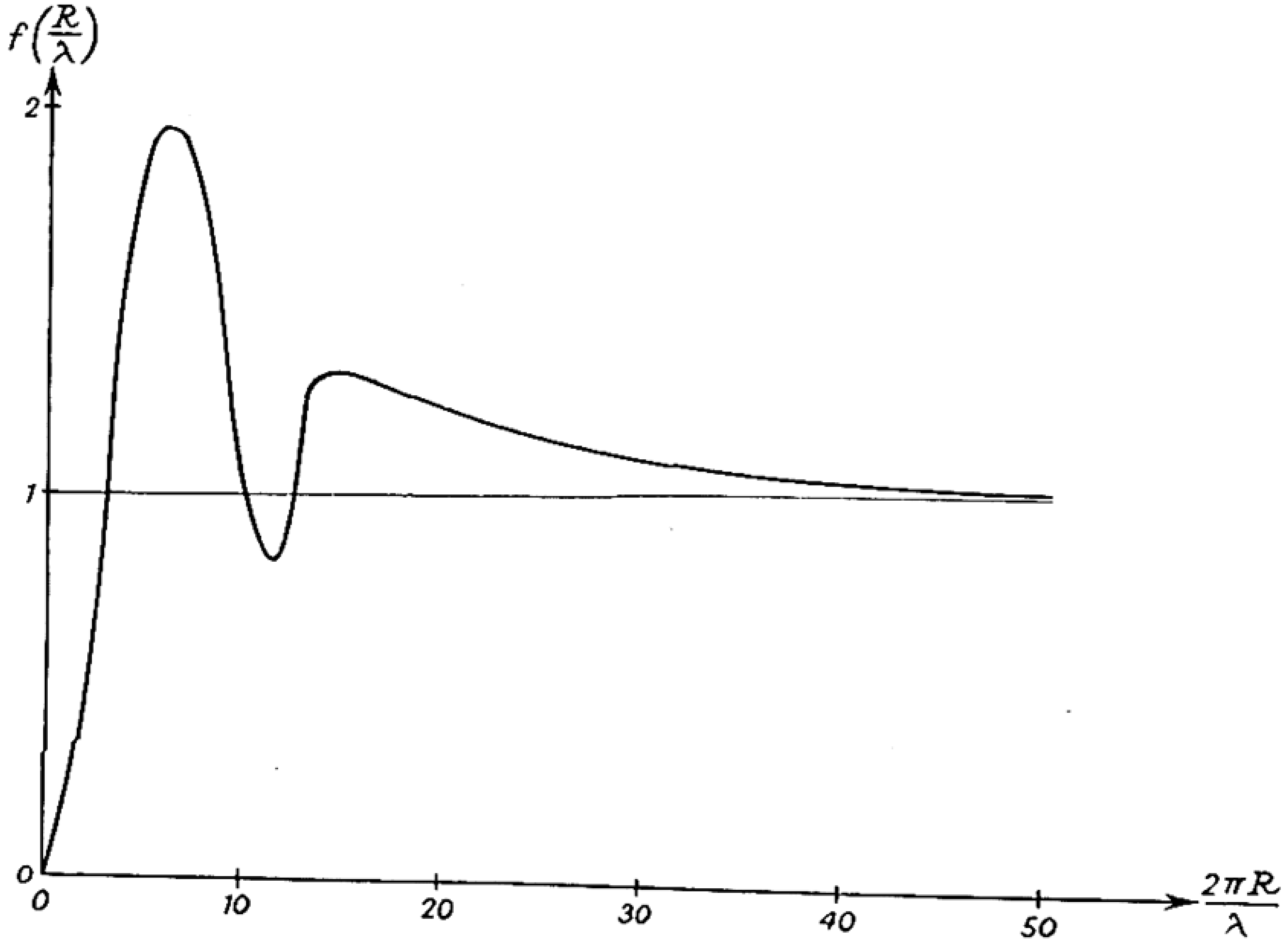

Figure 1.*

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* Since this work was written, we learned that the calculation made by Stratton and Houghton was recently ameliorated by Houghton and Chalker (*J. Opt. Soc. Amer.*, Nov. 1949, p. 955). In the first evaluation Stratton and Houghton had used Mie's relations in the case of small values for the parameter α and the Jobst's asymptotical expression for the higher values of this paramater (*Ann der Phys.*, 1925, 76, p. 863). Ryde (Meteorological factors in radio waves propagation. *Phys. Soc.*, London, 1946, p. 169) criticized the use of this asymptotical expression, and van de Hulst (Dissertation, Utrecht 1946) showed that it was incorrect.

The calculation made by Houghton and Chalker shows that the asymptotical curve represents in fact average values of the function f which oscillates around these. The fog we are considering in this work not being homogeneous, these oscillations cancel each other, and the average effect is only one to show up ; therefore, the use of the improved calculations does not modify appreciably the results obtained here.

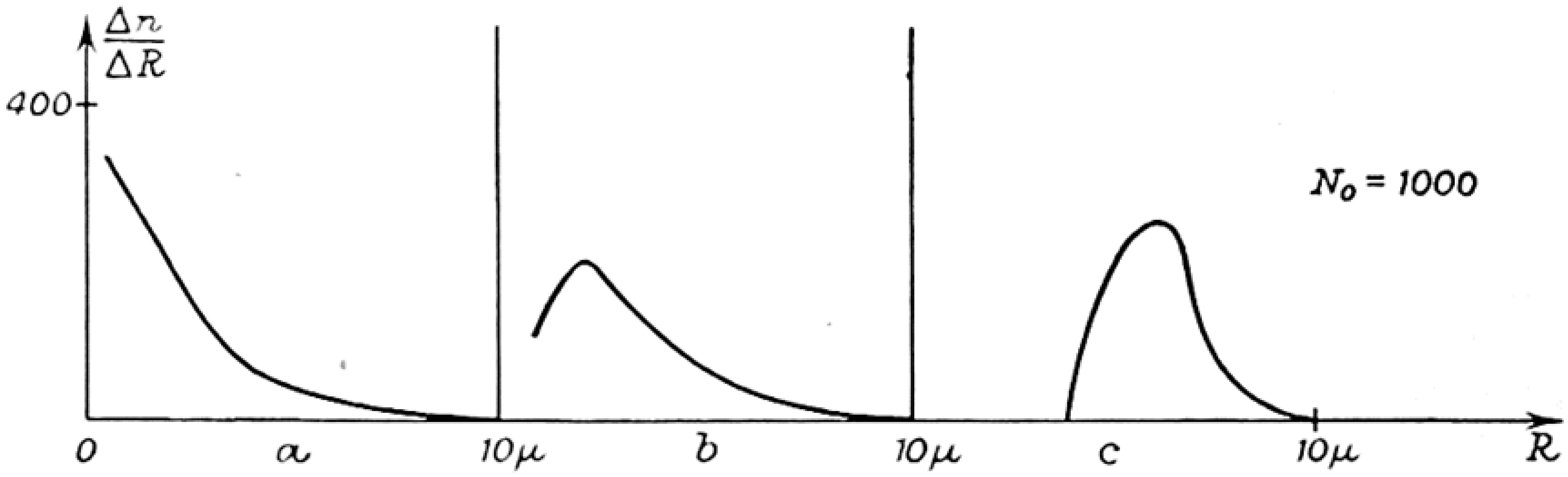


Figure 2.

(Bricard 1946) using visible radiation. We see that $\Delta n/\Delta R$ presents a maximum around 3μ. This maximum does not exist on the curve of Fig. 2 at *a*, for the second series (Arnulf, Bricard and Veret 1950) using the infra-red spectrum. On the same figure (*c*) is the distribution curve of the drops in a stratocumulus represented by Fig. 3, obtained during earlier experiments performed on the top of the Puy de Dôme (Bricard 1939, 1940). It is clear that natural clouds and those we have studied here have quite different constitutions ; it would be unwise therefore to extend the following results to natural conditions. In any case, the absorption coefficient K has, for a given wave-length the expression :

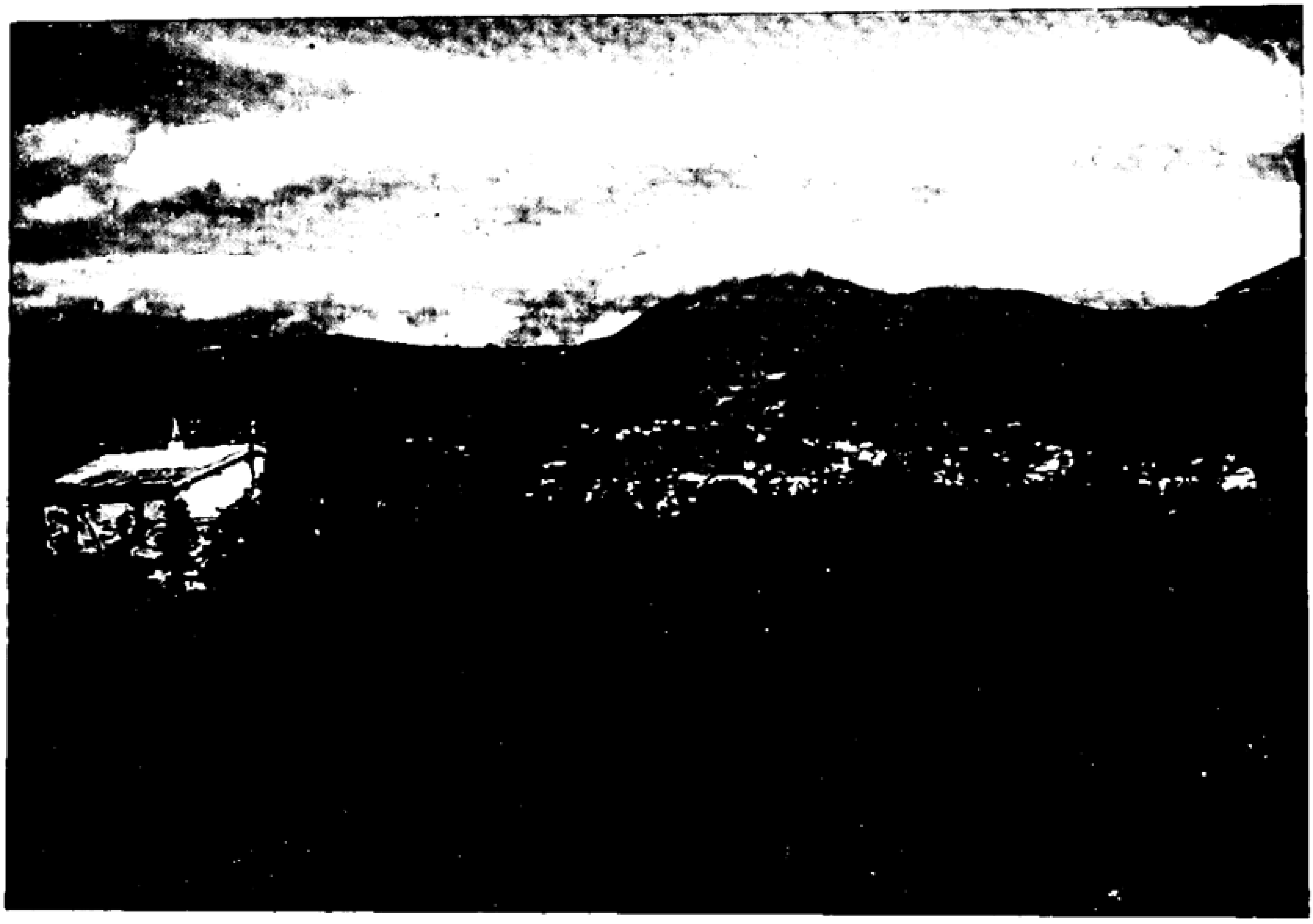
$$K = \frac{N}{N_0}\, \Sigma K(R)\, \frac{\Delta n}{\Delta R}\, \Delta R \qquad (3)$$

In addition, from the measurement of the mass of water per unit volume, we can easily deduce the ratio N/N_0. The same calculation for various wave-lengths enables us to know how K varies as a function of λ.

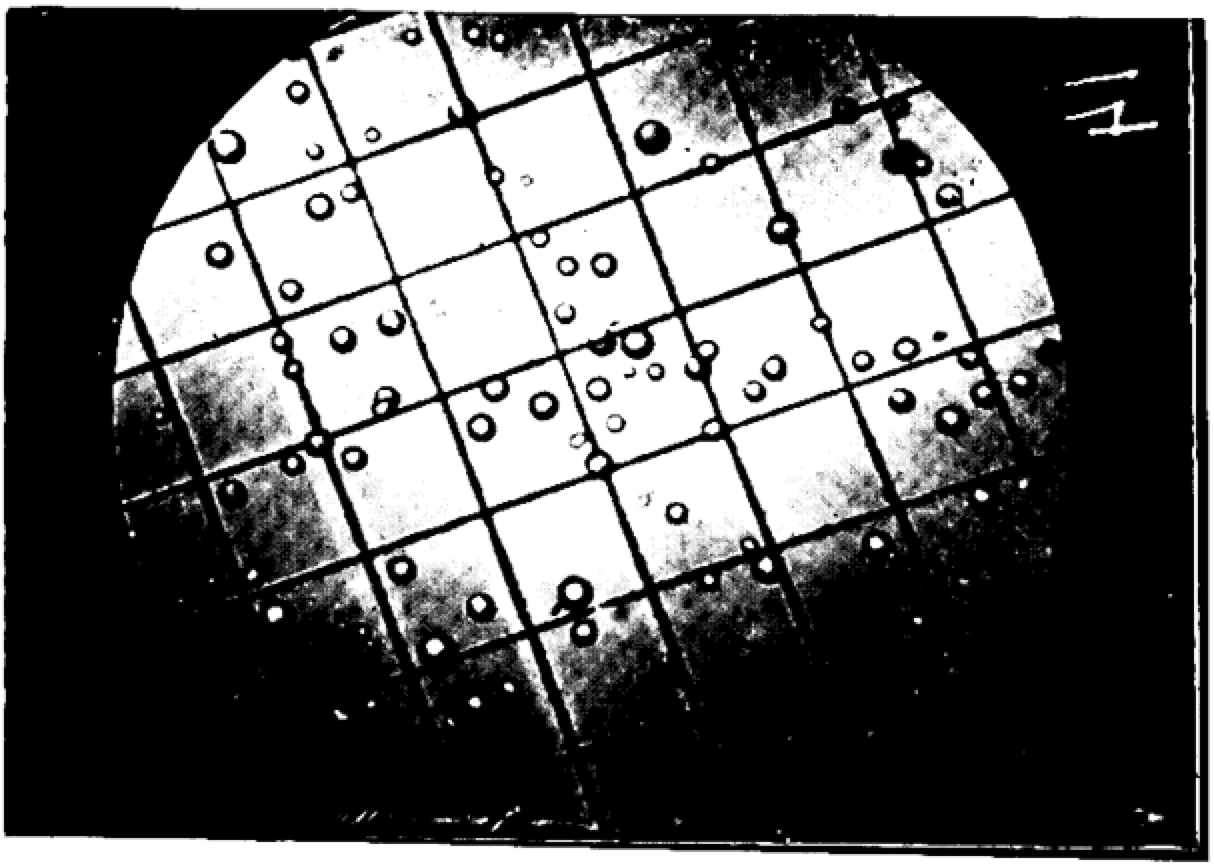
Houghton's (1931) measurements, confirmed by Nukyama and Kobayasi (1933), may be considered as a qualitative verification of the calculations of Stratton and Houghton. These authors studied the variations of the transmission factor of a fog, whose drops were of known average dimensions, and found a maximum absorption for the same wave-length as the one given by the theory. It does not seem that they took into account the distribution of the drop radii, which nearly explain certain differences between theoretical and experimental results.

In 1946, during a first series of measurements, I intended to determine on one hand the transmission factor for two different radiations, and on the other hand to characterize the fog by the drop-size distribution, and the mass liquid water per unit volume ; this gave a direct verification of calculations of Stratton and Houghton thanks to Eq. (3).

The fog was made in a closed tank provided with windows for the passage of the light to be studied. The measuring apparatus was placed outside, and all necessary precautions were taken to avoid errors due to the use of the windows. The average mass of liquid water per unit volume was determined by pumping a known volume of the air through a small tube T containing glass wool, the air of the tank being previously saturated and the tube being weighed before and after the operation. The dimensions of the drops were measured under the microscope, after having caught them in oil. Among the drops composing this fog, there were



(a) The top of the Puy de Dome in a stratocumulus cloud



(b) Corresponding drops

Figure 3.

some whose radius was less than 2μ. These drops would have escaped uncaught, if we had used only a mechanical catching device, such as exposing an oiled glass plate to a draught carrying the fog. We have secured the capture of our drops by utilising an electrostatic method (Brun and Pauthenier 1943) involving an electrical field and ionised medium ; after having captured the ions, the drops were driven into the oil, under the action of the field.

The source of light was a mercury vapour lamp, provided with filters permitting the isolation of the green line as well as the whole of the blue and violet lines. The determination of the transmission factor was effected by comparing the flux after the passage of the radiation through the fog to the flux obtained in the absence of the fog. This was done with a Fabry (1945) photometer, provided with a photoelectric cell. By aligning the axis of the photometer along that of the projector, one receives in this direction the total flux ϕ_1 which emerges from the fog ; by orienting it in a slightly different direction one receives only the flux ϕ_2 diffused by the drops. (It is found that this flux does not vary very much when the angle between the two axes remains sufficiently small.) The flux $\phi_1 - \phi_2$ will determine the flux transmitted.

The values of K were deduced from the photoelectric measurements and Eq. (1) ; values of K were calculated by means of the Eq. (3), where the condensed mass of water per unit volume and the drop-size distribution curve were known quantities, are in good accord with the experimental values (better than 12 per cent).

It seemed interesting to extend this verification of the theory given by Stratton and Houghton to the infra-red region. A previous study of the atmospheric transparency in this spectral region was made by Arnulf and Bayle (1949) without, however, a characterisation of the corresponding atmospheres. Arnulf, Veret and I (1950) undertook the study of the transparency of artificial fog in a spectrum extending from 0·4 to $5{\cdot}5\mu$. We used an optical system different from the preceding one ; the system was so designed that it could be used with artificial fog or in the open air. For this purpose, two parallel beams of light issuing from the same source (electrical arc) proceeded through the fog along two distinct paths of length e_1 and e_2 ; the shorter path was used as reference (Fig. 4).

Between 0·4 and 1μ we used a spectrograph with a glass prism, and a photoelectric cell. We projected successively the two beams on the cathode of the cell, the resulting current being examined with an oscillograph. Between 0·9 and $5{\cdot}5\mu$, we used a rock-salt prism and a thermoelectric cell, one beam illuminating one set of junctions and the other beam the other set of junctions.

The diameters of the fog drops were determined by microphotography after being caught on thin fibres. In order to avoid the evaporation of the drops during this operation, it was necessary to work in the fog itself, and in a place where the air was not in motion. We used the " plexiglas " network systems of L. Demon (1948) with a tight structure, so that most of the drops striking them were retained (Fig. 5). The diameters of the drops lay between 1 and 20μ. (The plexiglas threads were on average a few microns apart, and Demon's experiments indicate that all drops with a diameter larger than this are certainly caught.) There is at the most a loss of 10 per cent among the drops of about 2μ radius, and 20 per cent

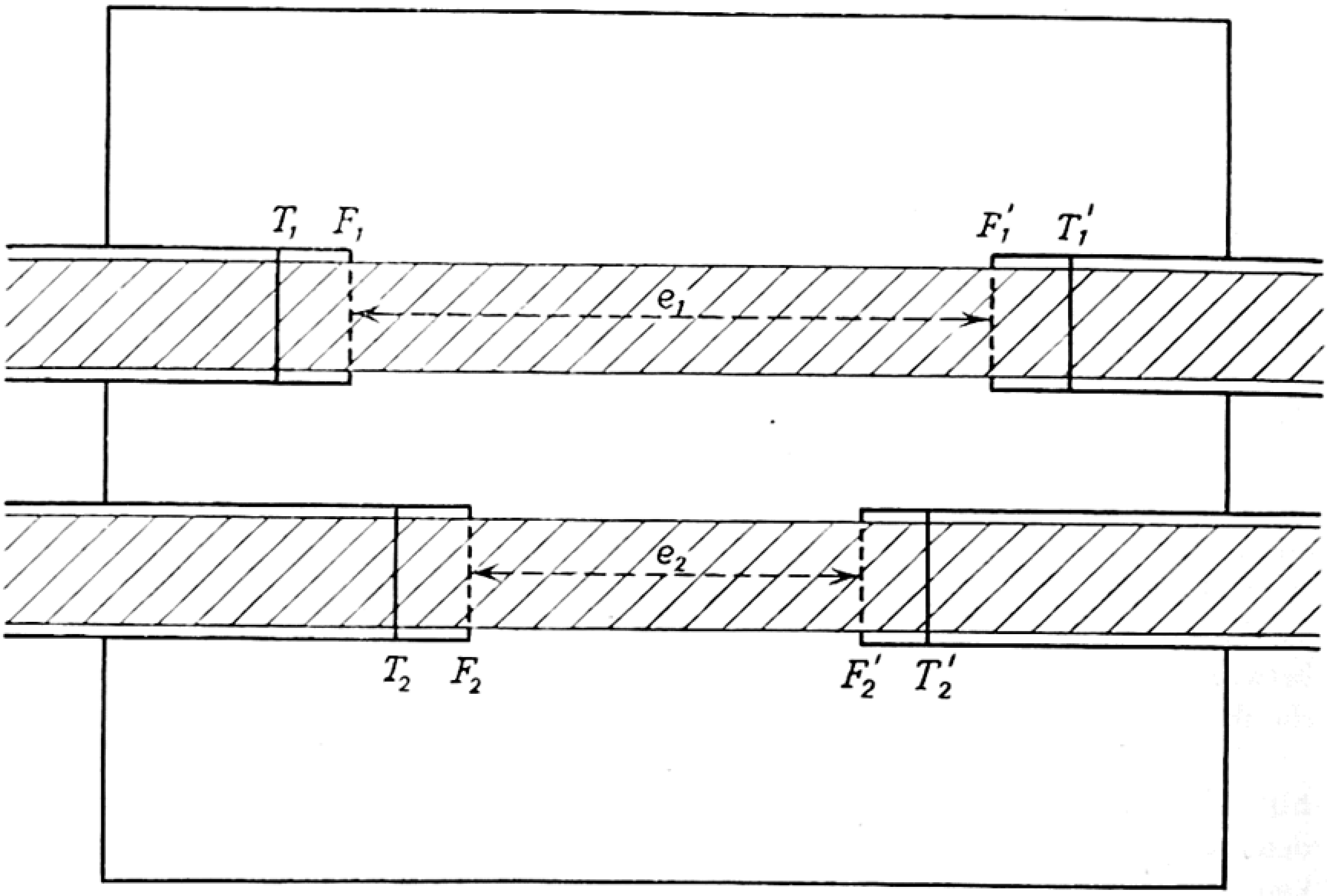


Figure 4.

among those of about 1μ ; these last are not very important for their optical effect. Fine drops were chosen because we wished to use the highly bent part of the curve given by Stratton and Houghton, which corresponds to lower values of the ratio $2\pi R/\lambda$; on the other hand, we had to operate with a medium low optical density because the experiments did not discriminate between transmitted and diffused flux, and we had to choose conditions where the latter could be neglected.

The difference $e_1 - e_2$ was 1 m, and for such a path there was no evidence of the bands of absorption by water vapour. The stability of the fog was controlled during all the experiments by observing the absorption given by a constant monochromatic light. This control having revealed noticeable fluctuations of the optical density during the twenty minutes needed for each series of measurements, it was necessary, before any interpretation, to calculate for each wave-length the average values given by all the corresponding individual experiments. These values are expressed in optical densities in Fig. 6 for visible radiation and in Fig. 7 for the infra-red.

The most important result is the constancy of optical density in all the parts of the spectrum. Knowing the distribution of drop diameters, we have estimated by means of Eq. (3), the relative variation of the absorption coefficient K, or what is the same, the optical density, as a function of the wave-length. (The calculations of the absolute values would necessitate the determination of the amount of condensed water per unit volume. This has not been done here.) The variations are represented by dotted lines in Figs. 6 and 7 ; they are quite different from those

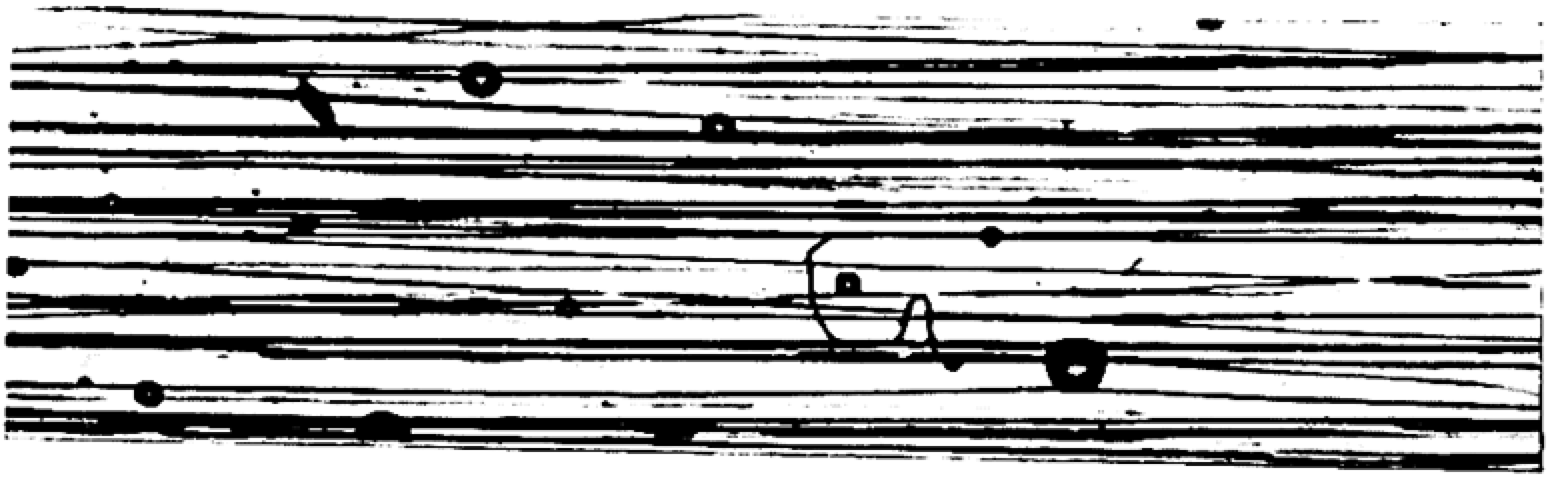


Figure 5. Demon's plexiglas network showing how much the dimensions of the drops can differ in an artificial fog. In order to avoid possible coagulation in the measurement, networks are used which have not been exposed for a long time and which have therefore only a few drops.

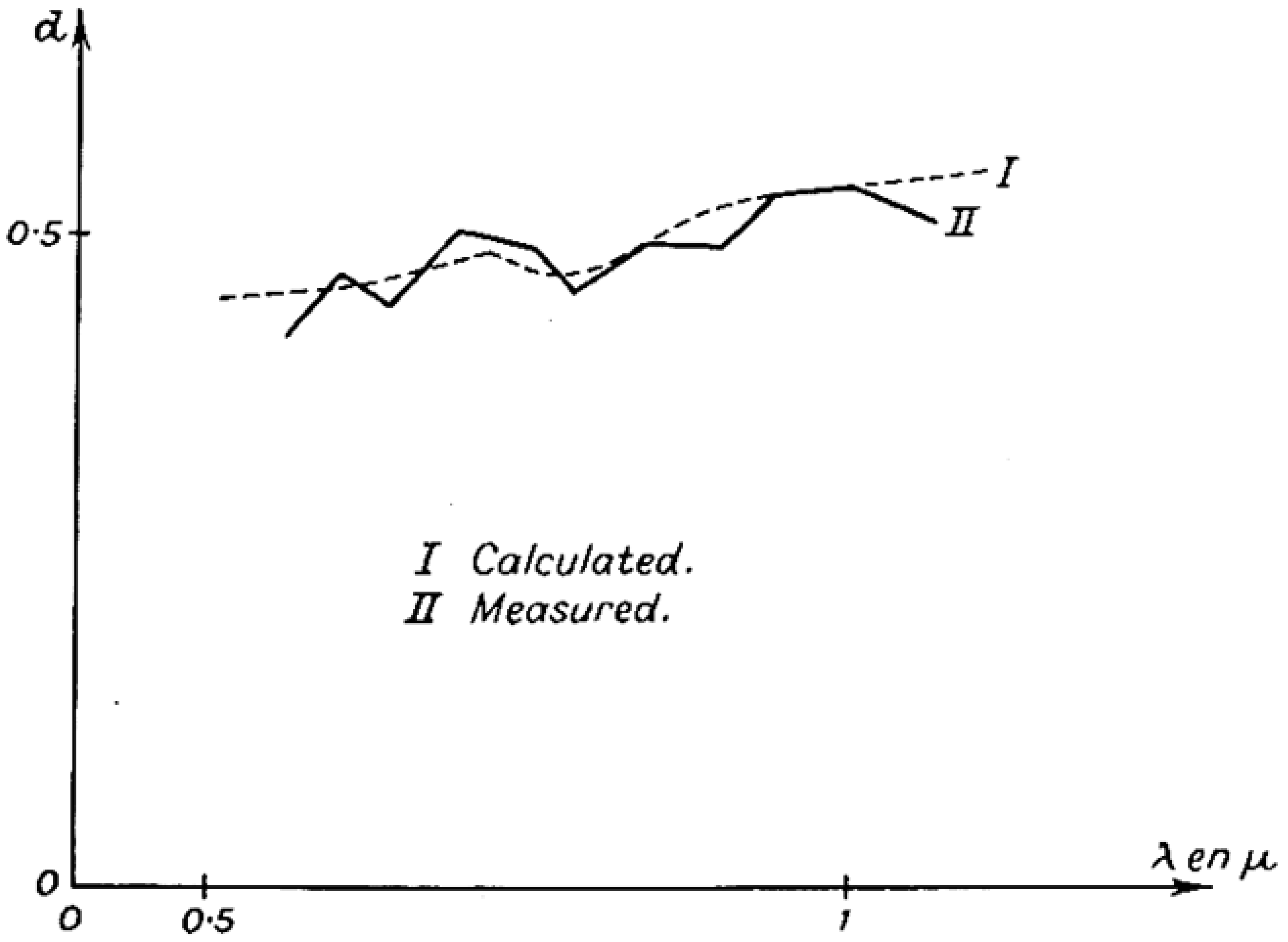


Figure 6. Optical density d as a function of wave-length.

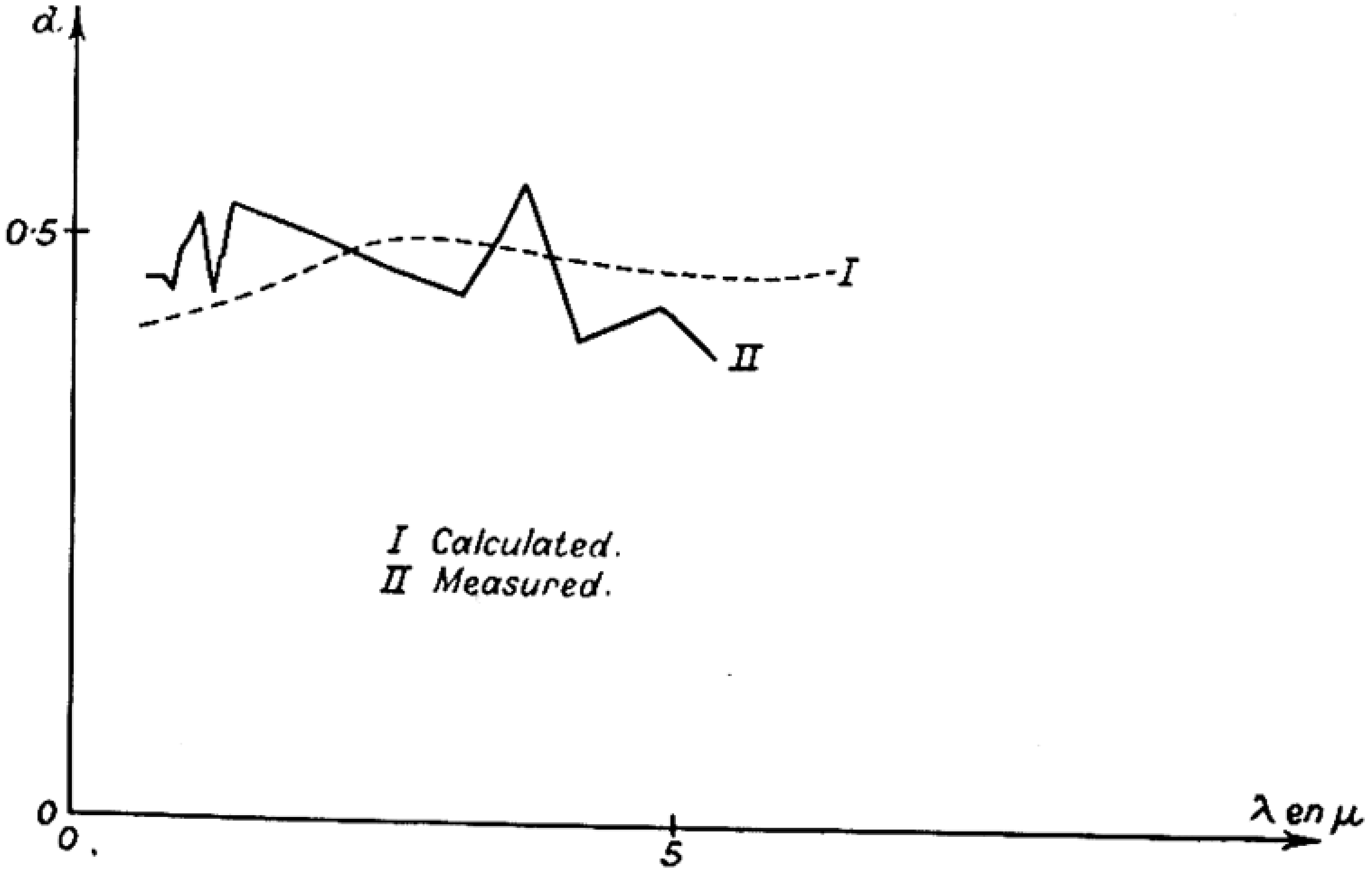


Figure 7. Optical density d as a function of wave-length λ in the infra-red.

which would result from a fog of drops all of the same diameter. One can see that the experimental values are quite near those we have deduced from the theory. However, differences appear in the infra-red region, but can be explained by the errors of measurement.

Driving, Mironov, Morozov and Khovostikov (1949) made similar measurements in natural fog with wave-length less than $0{\cdot}9\mu$; they measured the diameters of the drops, and found that the transparency of the fog was greater in the long wave-lengths, this being in contradiction with the theory of Stratton and Houghton; they suggested in explanation the presence of very many undetected small particles. Our results give an indirect confirmation of the possible presence of such particles. It would be interesting to detect them directly.

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The effects produced by seeding supercooled clouds with dry ice and silver iodide

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1. Historical

In 1943 a series of basic studies was inaugurated in our laboratory directed toward a better understanding of the causes producing precipitation static on aircraft radio. This work was carried out in the Mohawk Valley of eastern New York and at the summit of Mt. Washington at the Observatory. It was found (Schaefer 1947) that one of the most important charging mechanisms responsible for the development of static electricity on airplanes flying through snow storms was the breakage of the individual snow crystals as they struck the exposed surfaces of the aircraft.

In the course of making our precipitation static measurements on Mt. Washington, we encountered much difficulty with the icing of our collectors during stormy weather with temperatures below 0°C. Fortunately, we experienced occasional periods with high winds and blowing snow without clouds which permitted us to complete our part of the precipitation static problem.

In the course of these studies, we became much interested in the icing deposits produced when supercooled clouds made contact with exposed surfaces, and for the following two years, Dr. Langmuir and I devoted much of our time to various phases of this interesting subject. This work, which was carried out for the U.S. Air Forces until the middle of 1946, covered a number of subjects which have been summarized elsewhere (Schaefer 1946). At the end of the war, the studies were continued by the writer as a Research Laboratory activity, the emphasis of the research gradually shifting to the properties of supercooled water deposited as drops on solid surfaces having special surface properties. Subsequently, all of the studies were made with a supercooled cloud of water droplets floating in a cold chamber.

The earlier work was carried out with highly polished metal surfaces coated with various substances, some of which prevented the occurrence of supercooling and others which greatly favoured its development. It was discovered, for example, that a monomolecular coating made of a protein, such as pepsin or egg albumin, placed on a polished chromium surface would develop a thin frost coating which would instantly seed condensed water drops as they cooled below 0°C. On the other hand, a very thin coating of polystyrene or a silicone oil or resin would permit supercooling to temperatures below minus 20°C. In subsequent work along this general research problem, Smith-Johannsen (1948) in our laboratory was able to reach a temperature of —38·5°C before the ice phase was initiated.

Early in 1946, I decided to abandon the study of supercooling on solid surfaces since it obviously involved a number of features which were not characteristic of natural clouds in the atmosphere. Accordingly, a cold chamber was made from a 4ft^3 home freezer. Many experiments were devised which centred about a cloud of supercooled water droplets. Simple measurements showed the entire feasibility of reproducing in the laboratory a small segment of any type of cloud that could form in the free atmosphere. Subsequent experiments in the summer of 1946 led to the discovery (Schaefer 1946, 1948a) that dry ice could be used to form tremendous numbers of ice crystals in any air sample having a temperature lower than 0°C which contained a supercooled cloud or was supersaturated with respect to ice.

Subsequent studies (Schaefer 1949a) show that many fascinating experiments could be devised to demonstrate the physical and chemical relationships which occur in air containing moisture and various kinds and concentrations of condensation and sublimation nuclei, traces of surface active vapours, *etc.*

As soon as the dry ice effect was discovered, it was obvious that important changes could be expected when small quantities were dropped into supercooled clouds. This effect merely employed the Bergeron (1933) mechanism, but it implemented the suggestions advanced in Findeisen's important paper (1938) which predicted that profound changes could be produced in unstable cloud systems when triggering mechanisms were discovered.

In November 1946, the first attempt to modify a natural supercooled cloud was made by the writer and the successful transformation of the cloud from water droplets to ice crystals was repeated a number of times in an effort to establish this method as a practical method for initiating important changes in meteorological processes (Schaefer 1947b, 1948b).

It soon became apparent that research with atmospheric clouds was a large undertaking, and in March 1947, a joint study programme termed Project CIRRUS was inaugurated and supported by the U.S. Army Signal Corps, the Office of Naval Research, and the U.S. Air Forces with a General Electric Research Laboratory group serving as consultants and research workers.

Since that time many interesting advances have been made by the Project CIRRUS group and others in the field of experimental cloud studies (Anon 1948; Smith 1949; Orr, Fraser and Pettit 1949). Now termed Experimental Meteorology, rapid advances are being made on a wide front (Schaefer 1950). It is far too early to make generalized statements as to what can or cannot be done in this field. There are obvious limitations, but there are also many important possibilities, some of which have already been achieved.

2. SCOPE OF PRESENT FIELD AND LABORATORY STUDIES

If it were not for the common formation of supercooled clouds in the free atmosphere, there would be little opportunity for modifying clouds as based on our present knowledge. Supercooling develops because of the lack of sufficient concentrations of ice nuclei in the air where such clouds form. More than 6,500 observations in North-Eastern America (at the summit of Mt. Washington, 6,300 ft above M.S.L.) have shown conclusively (Schaefer 1949b) that the concentration of such particles varies from less than $10/\text{m}^3$ to more than $10^7/\text{m}^3$. About half of the time, the concentration is less than $10^4/\text{m}^3$. It is now considered by our group that a concentration of at least $1 \times 10^4/\text{m}^3$ is required if cloud transformations are to occur and persist. It can easily be shown that a concentration of ice nuclei one thousand-fold greater than $1 \times 10^4/\text{m}^3$ must be present if a rapid change from water droplets to ice crystals is to take place. If a chain reaction develops due to the break-up of a few primary ice crystals producing fragmentation nuclei, high concentrations of ice nuclei due to natural processes may occur. It requires from five to thirty minutes, however, under ideal conditions for such a process to develop enough snow particles to be effective in transforming a cloud into a precipitation area. It is because of the variability in the concentration of natural ice nuclei, the lack of particles effective in the range of 0°C to −12°C, and the ability we have to exceed greatly the maximum concentrations present in the free atmosphere that we believe that profound changes and anomalous effects may be achieved in cloud systems if they are seeded in an intelligent manner.

In the course of the past three years, our Project CIRRUS aircraft have completed approximately 150 experiments. All operations have been designed to obtain specific types of data, to try out new ideas, to explore the feasibility of carrying out a certain type of operation, or to test new instruments. All flight experiments have been purposely planned as small-scale operations in order that the data obtained would not be too complicated. A detailed description of the procedures and schedules followed in such operations has already been published (Schaefer 1950).

Among the studies which have been made are the following:

(a) The effect of seeding supercooled stratus clouds with various amounts of dry ice and silver iodide.

(b) The optimum quantity of seeding agent required to produce large cleared areas in an otherwise solid deck of supercooled clouds.

(c) The effects which follow the seeding of various sizes and types of cumulus clouds.

(d) The dissipation and re-growth of clouds in a seeded area.

(e) The relationship of seeding operations to the development of radar echoes in seeded cumulus clouds.

(f) The formation of snow areas in cold clear air supersaturated with respect to ice.

(g) The temperature at formation of cirrus clouds and the types of crystals which form in them.

(h) The rate of growth of snow crystals following the seeding of supercooled clouds of various types.

(i) The possibility of producing effects in large cloud systems by using a ground generator which is capable of producing 10^{17} ice nuclei/sec.

(j) The structure and nature of supercooled clouds in a hurricane.

(k) The structure and nature of warm tropical clouds which produce substantial amounts of rain without the Bergeron mechanism being involved.

(l) The varying concentration of condensation nuclei at altitudes from sea level to 35,000 ft obtained with a continuously recording nucleus counter.

(m) The use of a recording condensation nucleus meter for determining the entrainment within cumulus clouds.

(n) The measurement of true temperature within clouds above and below freezing.

(o) The possibility of modifying potential thunderstorms by various seeding techniques.

(p) Establishment of the effects produced by seeding supercooled clouds in various places with respect to air movement within the cloud, the freezing isotherm, and vertical thickness above the freezing level.

The field studies pursued under the Project Cirrus programme involve work on Mt. Washington, in the vicinity of eastern New York, and in such widely scattered places as New Mexico, Idaho, Honduras, Puerto Rico, and Switzerland. A large collection of lapse-time-motion pictures of all types of clouds have been secured which includes a remarkable pair of stereos obtained with two cameras on a 3/4 mi base line.

Besides this type of study, the field work has secured more than 6,000 observations of the concentration of ice nuclei in the free atmosphere, many months of records of the concentration of condensation nuclei, the latter involving a continuous sampling technique, a method for obtaining an automatic daily record of cloud types, several years' records of atmospheric electricity related to meteorological processes, methods for securing exact replicas of snow crystals, an improved method for securing rain drop samples, a series of studies related to the seeding of supercooled orographic clouds, and another series related to the seeding of supercooled radiation fogs. Many of these studies have been described in a preliminary way in the Occasional Reports of Project CIRRUS, and during the next year, it is planned that these studies will be summarized.

In the laboratory we have undertaken a wide field of investigation into such studies as condensation, sublimation, and freezing nuclei, crystallography, the dynamics of water drop and ice crystal formation and break up, the mechanism of entrainment, the relationship of electric charge to clouds of ice crystals and water droplets, and the development of methods for the quantitative measurements of atmospheric phenomena with new types of instruments.

3. SOME RESULTS

In the course of our flight studies, many interesting results have been observed. Few operations failed to produce positive results showing cloud modification which could be directly traceable to the seeding operations.

A few of these are illustrated in the accompanying photographs. Fig. 1 (p. 47) shows two lanes cut into a solid deck of supercooled clouds. The experiment was devised to test the feasibility of removing cloud cover blanketing the earth with a solid overcast. Using 0·5 to 1·3 lb of dry ice per mile of flight, it was found that the clouds could be dissipated quite effectively with each seeded lane growing to a width of two to three miles. Fig. 2 shows a view through a cleared region near the juncture of the two converging legs of a seeded pattern shaped like the Greek letter gamma. Some patches of new cloud are forming in the region which had been cleared by seeding. Fig. 3 illustrates another pattern cut into the clouds later in the day. Eventually, a large hole developed in the near foreground through which planes could pass without the use of blind flying aids.

Fig. 4 is typical of the brilliant optical effects which often follow seeding operations. It shows a reflected image of the sun and one of two reflected sun dogs, the other being 22° to the left of the sun pillar.

Fig. 5 illustrates an effect produced in cloud-free air which, however, was supersaturated with respect to ice. When small pellets of dry ice were dropped from the seeding plane, they produced ice crystals in tremendous numbers in the air in peculiar jagged lines. The circular strips at the upper edge of the seeded line are due to condensation from the exhausts of the airplane engines (a B-17). This snow area gradually thickened and became at least a mile wide and ten miles long persisting for several hours. When last seen, it was drifting with the wind toward the Berkshire hills.

An interesting result from a local dry ice seeding operation is illustrated in Fig. 6. A group of five cumulus clouds forming along the ridge of the Monzano Mountains of New Mexico were seeded at the top of the clouds with four pellets, 2 lb, 1 lb, 3/4 lb, and four pellets of dry ice respectively. Seeding occurred between 1344 and 1348 hr on 19 July 1949 at an altitude of 21,500 ft which was 5,500 ft above the freezing isotherm and 9,500 ft above the cloud base.

The first radar echo from the seeded area was observed about 15 min after seeding. A total of four separate precipitation regions were picked up by the radar, all within the seeded zone. At this time, there was no other precipitation observed

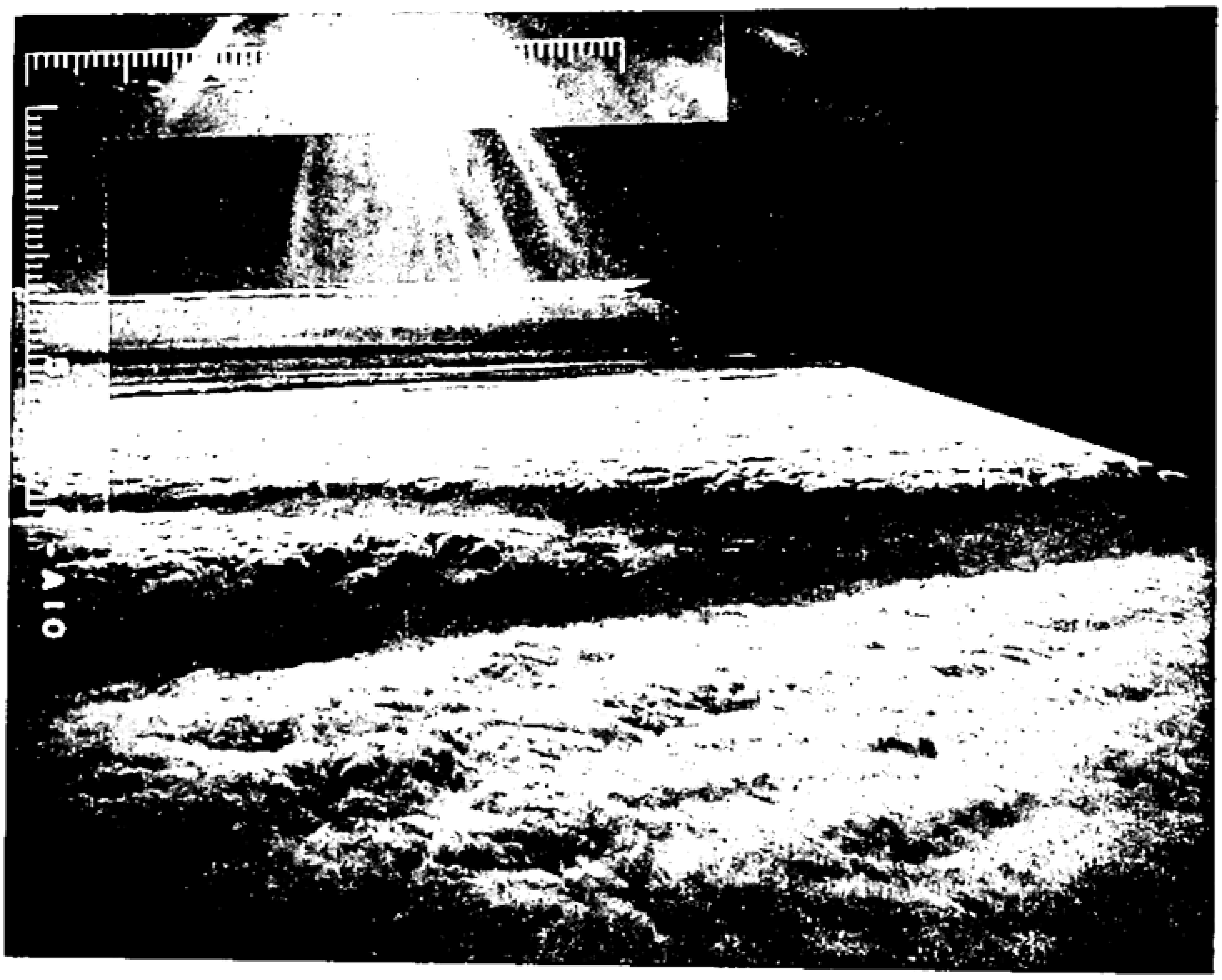

Figure 1. Effect produced in a deck of supercooled stratus clouds, temperature −4°C, by dry ice pellet seeding at rate of 1·3 pounds per mile of flight. About 10 miles of the 30 mile long legs of the " Gamma " pattern are shown. The lanes are about 2 miles wide. Flight 52, Project CIRRUS.

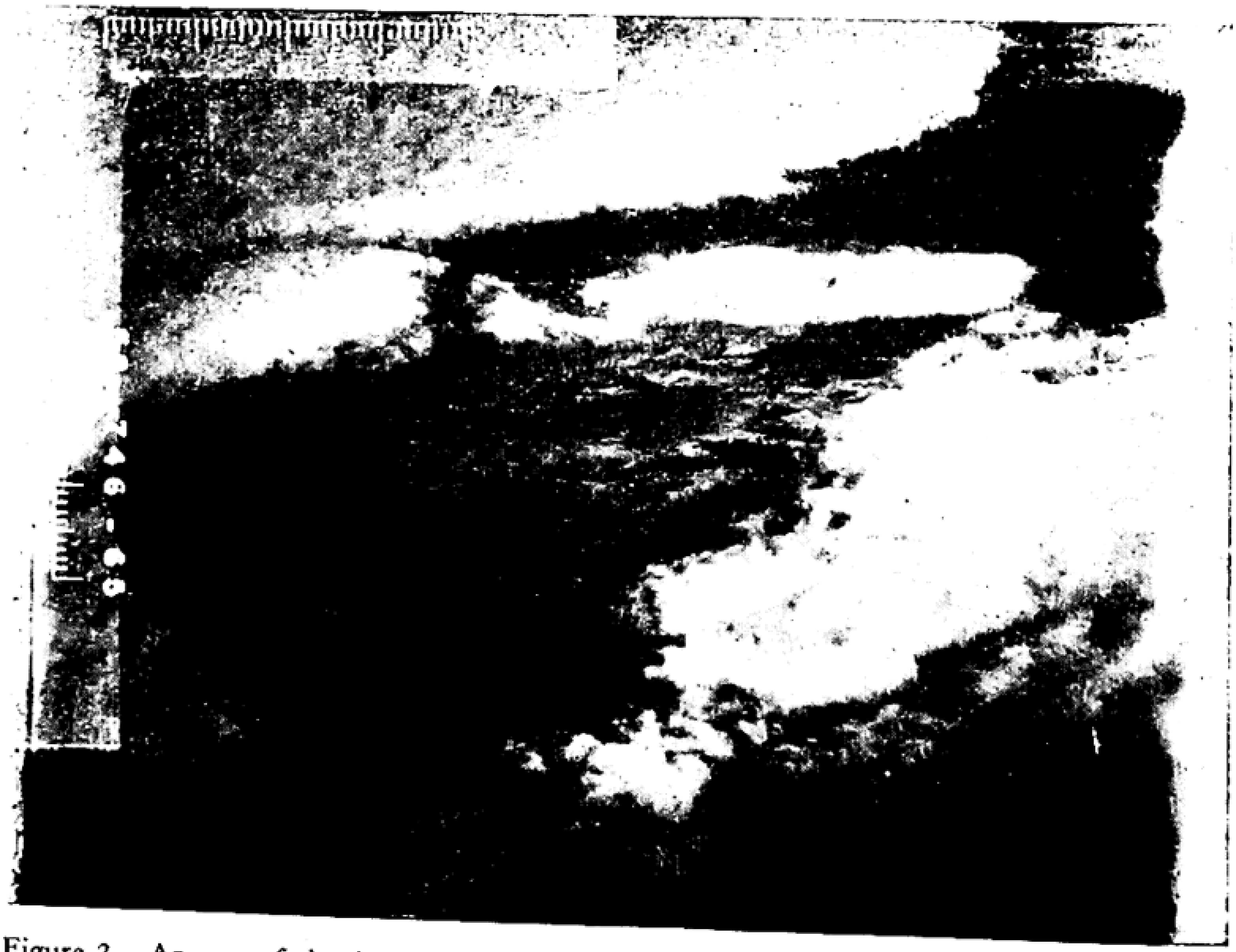

Figure 2. An area of cloud-free air produced by seeding supercooled clouds in a " Gamma " pattern. Flight 52, Project CIRRUS.

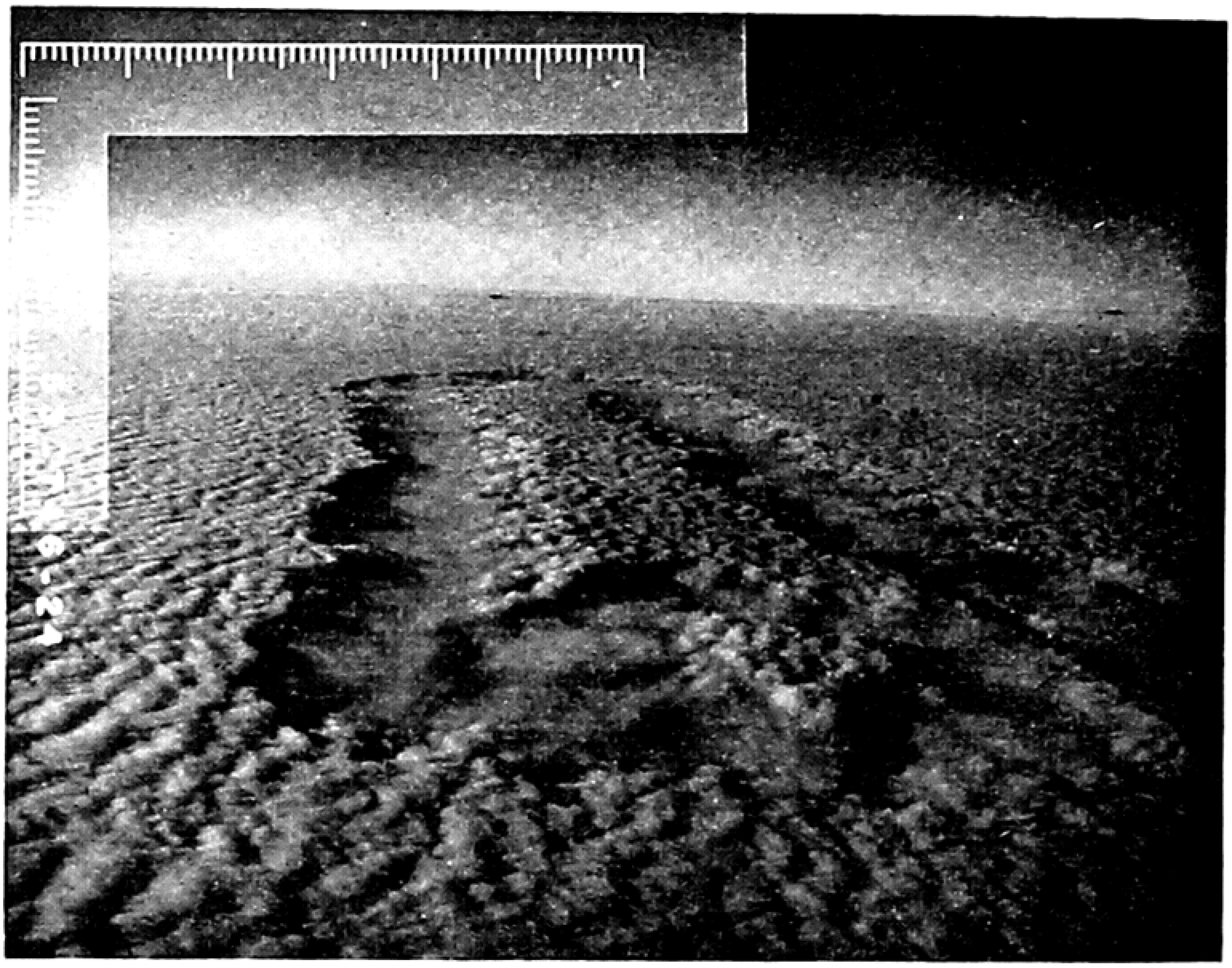

Figure 3. A deck of supercooled clouds modified in a " racetrack " pattern by seeding with two pounds of dry ice per mile of flight. Legs are about 18 miles long with an average width of 1·2 miles. 24 minutes after seeding. Thickness of cloud deck 2300 ft, temperature —4·6°C. Flight 53, Project CIRRUS.

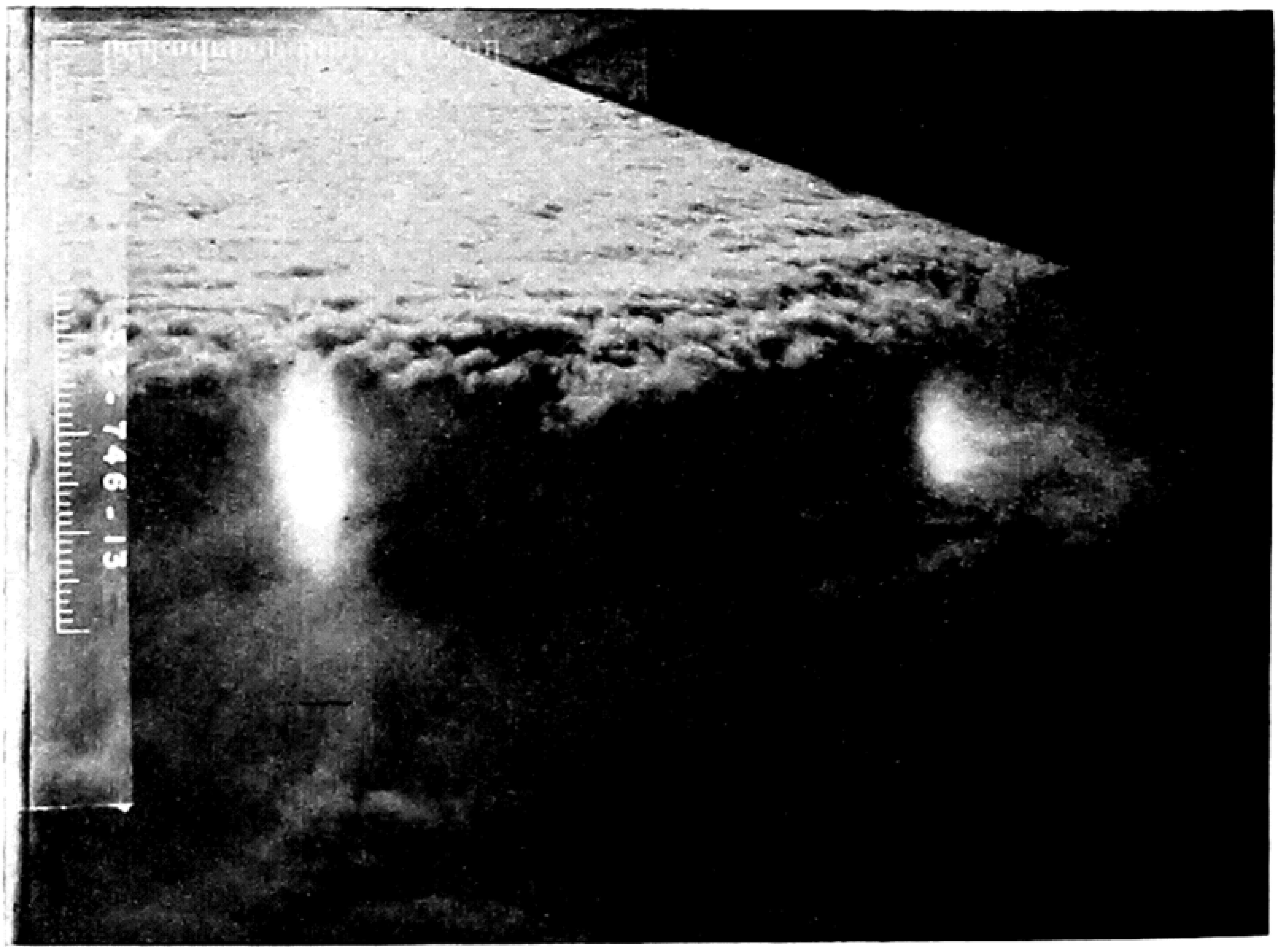

Figure 4. Optical effect observed in dissipating cloud area seeded with dry ice. The vertical streak on the left is a reflected sun pillar, while that on the right is a reflected parhelion at 22° from the sun's reflection. Flight 52, Project CIRRUS.

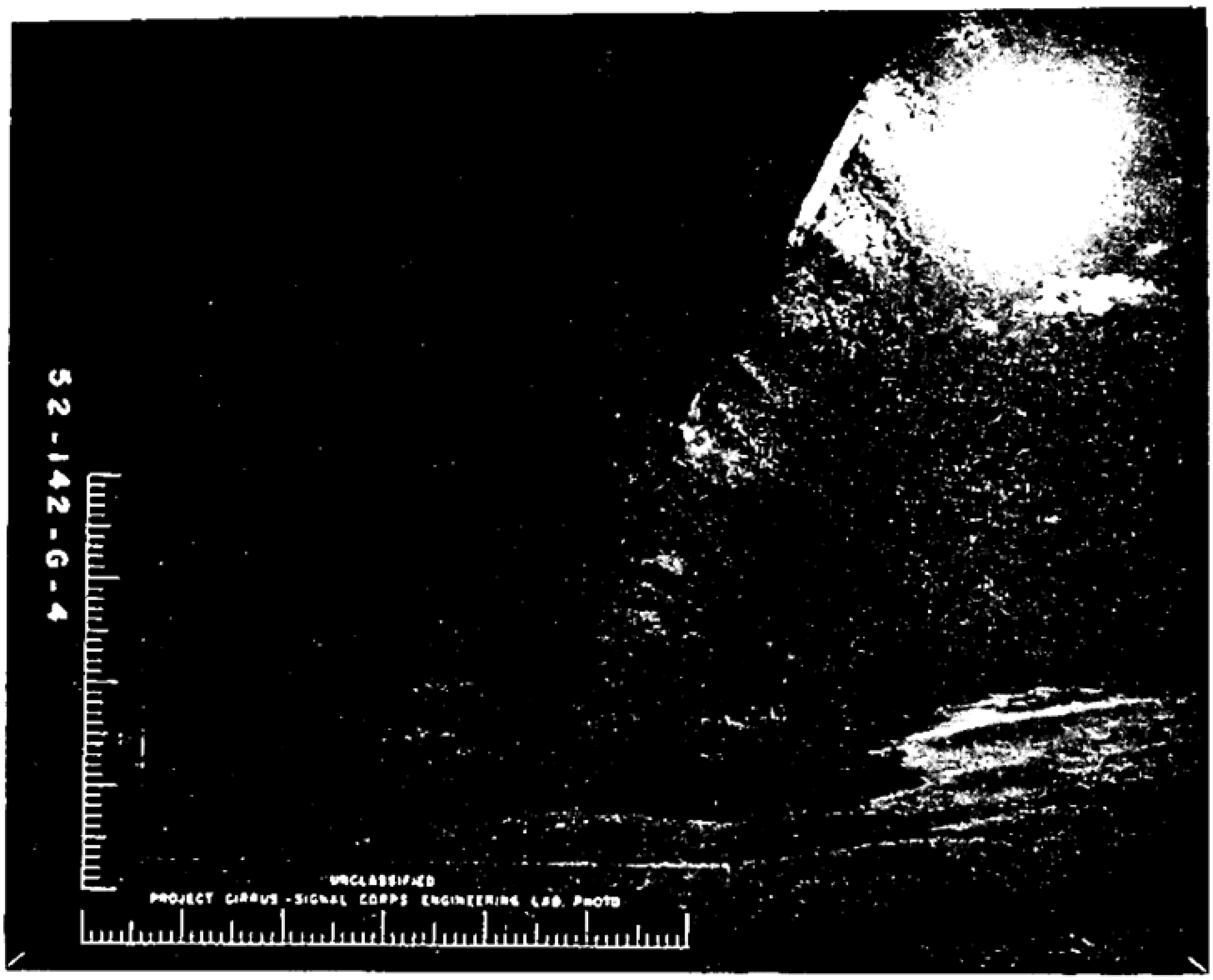

Figure 5. The production of a snow area in cloud-free air containing water vapour supersaturated with respect to ice. Note the seeded paths produced by single pellets. Track is ten miles long. Flight 142, Project CIRRUS.

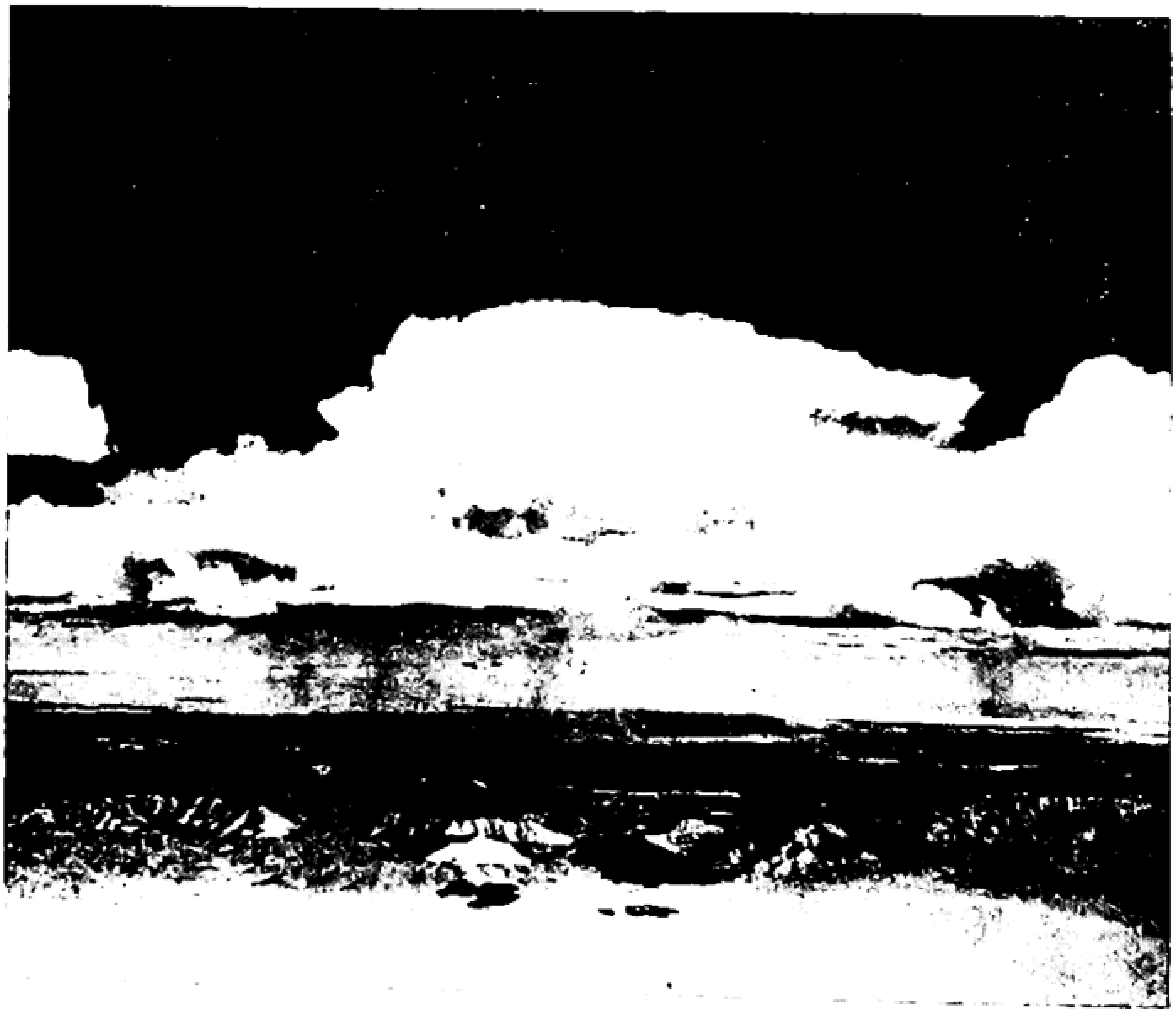

Figure 6. Rain areas below a large cumulus cloud which was seeded with dry ice. Precipitation areas developed and grew from the seeded areas as observed by radar and photographic studies. Nearest natural precipitation 113 miles. Flight 108, Project CIRRUS.

with radar within 60 mi and subsequent studies of precipitation records show the closest precipitation area to be 113 mi distant.

Although there were cumulus clouds over the Sandia Mountains which had the same general appearance as the clouds seeded over the Monzanos, none of the " control " clouds developed precipitation.

The rain which is falling from the cloud shown in Fig. 6 persisted for several hours and had a part in causing an increase in flow of the Pecos River. Toward evening a second storm followed the course of this earlier one. It is not yet established whether this rain was due to natural seeding or to some silver iodide which was known to be in the area in which this storm developed.

These figures illustrate the local and rather spectacular aspects which follow the seeding of clouds with dry ice or silver iodide.

4. Conclusion

In addition to producing such effects as illustrated, the new techniques which are rapidly developing in the field of experimental meteorology should be of immeasurable value in permitting the meteorologist to better understand atmospheric processes. For the first time it is possible to establish a starting time for studying a precipitation process on a grand scale. By carefully marshalling the many powerful tools now available for measuring the physical elements involved, it is possible to set up experiments in the atmosphere in the same manner as we are accustomed to make a bench experiment in physical chemistry.

Enthusiasm, imagination, and the other attributes which are essential elements in the carrying out of experiments in a new field are of great importance in the development of experimental meteorology.

If carried out in the proper manner, I am very sure the results will be most satisfying and I am equally confident that developments of great economic importance will eventually be realized in this important field.

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The formation of ice crystals and snowflakes

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SUMMARY

This paper contains a preliminary account of some results which have so far emerged from a theoretical attack on the problem of ice-crystal and snowflake formation in the atmosphere. The growth of an ice crystal from the vapour is discussed in terms of the new dislocation theory, the linear rate of growth being calculated as a function of ice-supersaturation, temperature and vapour pressure. An attempt is made to account for the various forms of ice crystal which predominate under given conditions, particular consideration being given to the transition from polyhedral to dendritic growth. The influence of the fall motion of the crystal on its growth form is discussed, and the coagulation of individual crystals into snowflakes is briefly considered. A more detailed treatment of these questions will appear later.

INTRODUCTION

Ice crystals and snowflakes have been collected, photographed and catalogued by a number of workers but little progress has been made in explaining how the various forms arise and what factors govern their growth. The reasons are not difficult to seek. The growth form of a crystal is partly determined by its basic structure and the nature of its intermolecular forces, and partly by the conditions prevailing in the surrounding vapour phase.

Until recently the crystal structure of ice was not known with any certainty as X-rays cannot locate the hydrogen atoms, and it is only with the introduction of the neutron diffraction technique (Wollan, Davidson and Shull 1949) that the position is beginning to be clarified. Ice does not possess a geometrically simple lattice so that calculation of the specific surface energies of the different crystal faces is very difficult.

The earlier attempts to correlate the frequency of various crystal shapes with the temperature at the place of observation were neither very successful nor informative — only the conditions prevailing during growth of the crystal are relevant. Such information has only recently become available as a result of the work of Weickmann (1947) during the war.

He has made a systematic study of snowflakes and ice crystals, both at ground level and at all heights in the atmosphere up to cirrus levels, in all types of clouds, and consequently in widely different conditions of temperature, ice-supersaturation and vapour density, the temperature of the sampling region being measured. The ice crystals were caught on a glass slide coated with a lacquer which, after a short exposure to the cloud elements, was brought into the aircraft and photographed. Weickmann's results are summarised in Table I.

He attempted some qualitative theoretical interpretation of his results using Volmer's ideas of crystal growth from the vapour, but as we shall see, Volmer's theory does not stand up to quantitative examination.

The purpose of this paper is to present a brief account of a new approach to the problem. A full account of the theory will appear later.

TABLE I. Classification of ice crystals according to shape, cloud type and environmental conditions. (Based on Weickmann's observations)

Crystal form	Cloud type	Conditions in sampling region Temperature (°C)	Humidity* (per cent) (w.r.t.ice)	Vapour pressure* (mb)
Hollow prisms (mainly clusters)	Cirrocumulus Cirrus castellatus Cirrus uncinus	−30 to −50	135 to 165	·50 to ·06
Complete prisms	Cirrostratus	−25 to −50	75 to 100 —W 95 to 165 —I	·60 to ·06
	Altostratus Altocumulus	−25 to −50	125 to 165	·80 to ·06
Thick plates	Altocumulus Altostratus	−15 to −25	115 to 125	1·9 to ·80
Thin plates	Nimbostratus Stratus Stratocumulus	0 to −15	100 to 115	6·0 to 1·9
Stars Needles† (Dendritic forms)	Nimbostratus Stratocumulus	0 to −15	100 to 115	6·0 to 1·9

* Last two columns calculated on the assumption that water saturation exists in all clouds except Cs.
† Needles are not included in Weickmann's observations.

2. Rate of growth of an ice crystal from the vapour

Every ice crystal growing in the atmosphere originates from a nucleus, the nature and behaviour of which have been discussed in a recent paper (Mason 1950).

Growth of crystals from the vapour has been treated theoretically by Becker and Döring (1935) and Volmer (1939), using the methods of statistical thermodynamics. The rate of growth as deduced from their theory is however vanishingly small and hence a revision of the theory is called for. These authors imagined a crystal face to be propagated parallel to itself by the formation of successive monomolecular layers, each layer starting from a two-dimensional nucleus of critical size formed on the surface by a chance aggregation of molecules. The formation of such a nucleus requires a fluctuation of free energy, the rate of growth being controlled mainly by the rate of nucleus formation. Once the nucleus attains its critical size, the layer can be completed comparatively rapidly. It can be shown that, according to the Becker and Döring theory of nucleation on a perfectly flat crystal face, an ice crystal would grow only at the rate of about 10^{-9} cm sec^{-1} or 1 μ per day, at ice-supersaturations as high as 50 per cent. Cabrera and Burton (1949) have shown that even if diffusion of molecules on the surface of the crystal is taken into account (an effect neglected by Becker and Döring), the rate of growth is not materially affected. Let us suppose however that the crystal surface is not perfectly flat but contains a number of molecular steps ; then growth could ensue by the deposition of molecules at the edges of the steps, the steps themselves acting as two-dimensional nuclei. If these steps were to disappear by the deposition of molecules the surface would become flat and growth would be greatly retarded.

For growth to continue, the steps must persist. A mechanism by which such steps can arise and persist during growth has recently been put forward by Frank (1949). He suggests that screw dislocations arise in the crystal due to the presence of impurities or other causes, and on terminating in the crystal surface produce a spiral terrace of steps which do not disappear during deposition or evaporation of molecules. The growth fronts spreading out from the dislocation form a growth pyramid, — in effect, growth takes place " up a spiral staircase." Growth essentially depends on the existence of kinks in the steps. The kinks are formed as a result of the evaporation and condensation of molecules and are illustrated in Fig. 1.

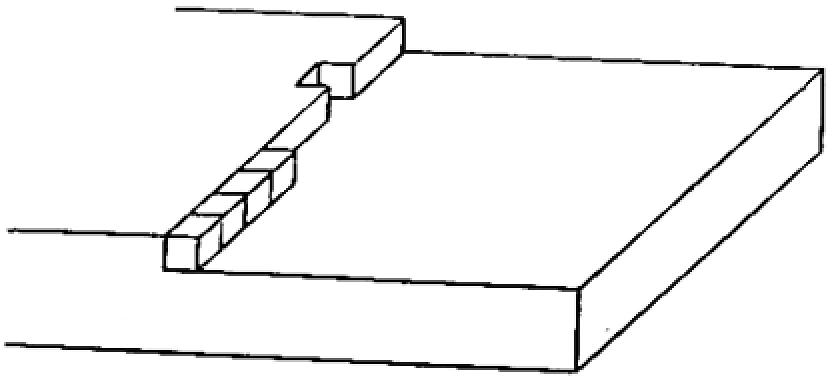

Figure 1. Schematic diagram of a step containing kinks on the surface of a crystal.

The energy required to remove a molecule from a kink into the vapour is approximately equal to the latent heat of evaporation of a molecule. It can be shown that no fluctuation of free energy is required for a molecule to be condensed into a kink ; further, if the distance which a molecule travels on the surface before evaporating is greater than the mean distance between kinks, it has a high probability of being built into the crystal structure. Under these circumstances, Burton (unpublished) has shown that the growth rate is largely independent of the kink density and the density of dislocations, provided that there is at least one per face, and is proportional to the supersaturation σ of the vapour, if σ exceeds a small critical value. The linear rate of growth is then given by

$$R = \sigma n v \nu e^{-W/KT} = \frac{A v \sigma p}{\sqrt{2\pi m K T}} \quad . \quad . \quad . \quad (1)$$

where v is the molecular volume, m the molecular mass, n the number of molecular spaces per unit surface area available for occupation, ν the frequency of vibration of a surface molecule, W the latent heat of sublimation per molecule, K Boltzmann's constant, T the temperature, and p the equilibrium pressure of the vapour relative to the crystal surface. A is Langmuir's " accommodation coefficient " assumed equal to unity.

If it is assumed that the crystals commence their growth at water-saturation, σ and p are fixed uniquely by T. Substitution of the relevant values in Eq. (1) shows that the maximum growth rate occurs at about −15°C and amounts to about $30\mu\ \mathrm{sec}^{-1}$, a much more realistic value than that predicted by the Becker and Döring theory.

Eq. (1) cannot be applied in general, however, to the growth of ice crystals in the atmosphere, when air as well as vapour surrounds the crystal. In this case the rate of growth is largely determined by the rate of arrival of vapour at the crystal surface which is controlled by the diffusion of water vapour in air. Eq. (1) will only apply when the dimensions of the crystal are less than the mean free path of the air molecules ($\sim 10^{-5}$ cm), which is of the same order as the diameter of the crystal nucleus. It gives in fact the maximum possible rate of growth ; in the general case, σ, the supersaturation in the undisturbed vapour, will have to be replaced by the supersaturation at the crystal surface. The foregoing theory does show however that any interpretation of ice-crystal growth from the vapour on the basis of the Becker and Döring — Volmer theory is of little value.

In the atmosphere, then, the rate of growth of a crystal will be determined by the rate of arrival of vapour at its surface and the rate at which the vapour molecules can be built into the surface structure in the manner described above. The relative rates of these two processes determine also the mode as well as the rate of growth, as we shall see later.

3. SHAPE DIFFERENTIATION OF CRYSTALS

Atmospheric ice crystals and snowflakes exhibit a wide variety of shapes and forms but the majority fall into two main groups : the unit crystals consisting mainly of hexagonal prisms and plates, and the dendritic forms which include the stars and the needles. Snowflakes and the more complex crystals generally consist of a combination of these basic forms.

(a) *Individual crystals*

The growth form of a crystal will clearly be determined by the relative rates of growth of the different crystallographic faces. The internal symmetry of the ice suggests that the dominant external forms will be hexagonal prisms, hexagonal bipyramids, and hexagonal prisms terminated at both ends by pyramids whose sides are inclined 27°57′ to the longitudinal axis. The pyramidal faces will tend to grow out however when the vapour phase becomes slightly super-saturated with respect to the crystal face, the bipyramidal form giving way to a hexagonal prism bounded by plane ends. Consequently, bipyramidal forms will be very rare and will not exist in clouds favourable for crystal growth, but they may occur occasionally in cirrostratus. Humphreys (1929) shows, in fact, that pyramidal forms are necessary to explain the formation of rare haloes seen occasionally in cirrostratus. Similar conclusions have been reached more recently by Weickmann (*loc. cit.*) who claims also to have found a few prisms with a single pyramidal end and advances this as an argument for the polar nature of ice. Examination of his photograph reveals however that these crystals contain central cavities which strongly suggest that they may have broken away from a cluster of hollow prisms which are predominant in isolated cirrus.

The most common growth form is then the hexagonal prism which develops as a result of preferential growth along the principle (c) axis, *i.e.* normal to the basal face (Fig. 2). The relative rates of growth on the basal and prism surfaces, assuming equal rates of arrival of vapour to both, can be written

$$\frac{R_B}{R_P} \sim e^{(W_P - W_B)/KT} \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

where W_P and W_B represent the amounts of energy required to remove a molecule from the prism and basal faces respectively. These energies have not been calculated and cannot be determined experimentally. There is indirect evidence however that W_B is less than W_P, as witnessed by the fact that ice cleaves preferentially parallel to the basal plane. Also, the coefficient of linear expansion is greater along the *c* axis than along the perpendicular *a* direction (Megaw 1934), indicating that the atoms are bound together less strongly in directions normal to the basal plane. Thus we should expect R_B to be greater than R_P, leading to preferential growth along the principal axis and development of prismatic crystals.

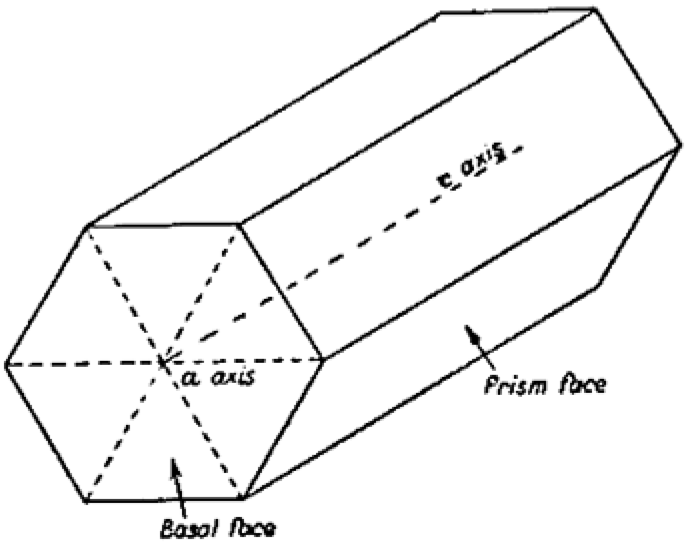

Figure 2. Hexagonal prism showing crystallographic faces and axes.

From Eq. (2) it is seen that the ratio R_B/R_P should decrease with increasing temperature, leading to more nearly isometric prisms at higher temperatures. For example, if $R_B/R_P = 5$ at —50°C, the ratio would decrease to 4 at —10°C. Weickmann's observations however point to a more rapid change of shape with temperature. He found a continuous transition from long, thin prisms at about —40°C through shorter, thicker prisms, to thick plates and finally to thin hexagonal plates, the latter predominating above about —10°C. This may perhaps be explained by a mechanism now to be discussed.

It appears that the thin hexagonal plates are formed as a result of growth on the basal surface being overshadowed by a much more rapid tangential development in the direction of the auxiliary axes. To explain this transition from prismatic to plate forms, Weickmann has suggested that, at higher temperatures, the molecules striking the basal face do not form a new layer but diffuse across the face and are built into the crystal structure only at the edges. This suggestion was prompted by the results of an experiment carried out by Volmer and Estermann (1921), who observed that the rate of growth of the edges of very thin mercury plates was about one thousand times that which could result by direct deposition of atoms from the vapour. There is other experimental evidence that diffusion of molecules from neighbouring surfaces can contribute to the growth rate of a given surface, and we should expect this contribution to be more important the smaller the dimensions of the collecting surface.

If we consider a thin plate of thickness *t*, the relative rates of growth by surface diffusion and direct deposition of vapour on the edge are given by $2\lambda_s/t$, where λ_s

is the mean distance travelled by a molecule on the surface before evaporating. Now $\lambda_s = de^{(W-U_s)/2KT}$ where W is the work required to remove a molecule from the surface into the vapour, U_s is the activation for diffusion on the surface, and d is the intermolecular distance. At temperatures not far below 0°C, U_s will be small compared with W, *i.e.* the surface molecules will have considerable mobility, so we can write $\lambda_s \sim de^{W/2KT}$. Putting $W = 8{\cdot}5 \times 10^{-13}$ erg, $T = 260^\circ K$, $d = 2{\cdot}76 \times 10^{-8}$ cm, we obtain the condition for surface diffusion of molecules to the edges to be the dominant factor, *viz* :—

$$t < 50\mu.$$

Now the thin plates which predominate in the temperature range —15°C to 0°C have thicknesses varying between about 10μ and 20μ, so that growth at their edges by diffusion of molecules from the basal face will be important. This process must have been greatly accentuated in the experiments of Volmer and Estermann where the thickness of the plates was only about $0{\cdot}1\ \mu$. The true ice plates which grow in this manner are generally quite small, being only about $100\ \mu$ diameter ; larger plates result by the filling in of stellar forms (see next section). The diffusion of molecules on the surface is reduced at lower temperatures, (presumably owing to the greater values of U_s), as indicated by the formation of 'glassy' ice on the thimbles of frost-point hygrometers at temperatures below about —70°C (Dobson 1949).

It is important to point out here that the growth form of a crystal may be profoundly modified by the presence of small quantities of impurity — adsorption of impurity at a growing step might well inhibit the growth of the crystal face in question. Such effects have been observed in a very striking manner by Schaefer.*

(b) *Dendritic forms*

Of the dendritic growth-forms, the snow stars which are often elaborately branched are the most striking. They generally arise from small hexagonal plates, the corners of which shoot out to form the arms of the star. Such a development marks the beginning of dendritic growth as opposed to the ordered development of crystal faces leading to polyhedral growth.

Let us consider first of all a plate crystal at rest in the surrounding vapour. Vapour is extracted from the field at the crystal surface, and a concentration gradient is set up in the vapour. There will be an initial tendency for the corners to sprout when diffusion first commences since they protrude into a region where the concentration of vapour is higher than at other points on the surface. This tendency soon becomes suppressed however as the diffusion field extends farther from the crystal and supplies vapour at all points on the crystal surface at a rate equal to that at which material is extracted and built into the crystal structure as described in Section 2. These conditions are requisite for polyhedral growth, and hold only when the growth of the crystal faces is relatively slow.

If this growth rate is large, the vapour cannot be brought from a distance by diffusion at a rate sufficient to replace that deposited at the crystal surface, so that the concentration near the crystal falls, and polyhedral growth breaks down since it would only increase the demand for vapour. Under these conditions only the corners develop, these pushing out continuously into new regions of higher concentration and the diffusion field becomes highly convergent towards the growing tip.

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* Reported in this Symposium.

If the crystal as a whole is at rest, the boundary between the region being denuded of vapour by the growing tip and the undisturbed field may be regarded as moving with a velocity approximately equal to the growth velocity R. In a later paper it will be shown that the condition for dendritic growth of the corners is

$$R > D/d$$

where D is the coefficient of diffusion of water vapour and d is the diameter of the plate crystal. A partial treatment of this problem has also been given by Wall (1947). The relative tendencies for dendritic and polyhedral growth can in fact be expressed by a dimensionless parameter $K = Rd/D$. Putting $D = 0{\cdot}25$ cm^2 sec^{-1}, $d = 0{\cdot}5$ mm, we have from the above inequality that R must exceed 5 cm sec^{-1} for the stellar forms to develop. But R is only of the order 10^{-3} cm sec^{-1} so that under the conditions just postulated the crystal would retain its polyhedral form.

But we have so far neglected the fall motion of the crystal ; plates of the order 0·5 mm diameter have a terminal velocity of about 30 cm sec^{-1}, and as they fall the diffusion field will be disturbed at a rate approaching this velocity. Diffusion will be limited to a thin boundary layer, which is continually renewed, the concentration gradient being greatest at the edges and corners of the crystal. As a first approximation we may regard the crystal at rest as above, but replace R by the velocity v of the crystal relative to the air.

The condition for stellar formation of our plate can then be written

$$vd > D$$

Thus if $D = 0{\cdot}25$ cm^2 sec^{-1} and $v = 30$ cm sec^{-1}, dendritic growth will set in only when d exceeds about 0·1 mm. This is in good agreement with observation.

Whether or not the star development will be well marked will depend on the vapour density of the cloud and the concentration gradient towards the corners — richly branching forms will occur only when these quantities have high values. Often the spaces between the arms of the star become filled in to form a large plate ; this may occur when the star encounters conditions unfavourable for further dendritic growth, *e.g.* when it falls into a region of lower concentration. One unit often shows several such transitions from star to plate-like growth (see for example Bentley and Humphreys (1931)).

4. FORMATION OF SNOWFLAKES

This important topic will be dealt with only very briefly in this preliminary paper.

Snowflakes consist of a rather haphazard collection of loosely interlocking crystals, the components being mainly stars and needles. The individual components can often be identified and it can be inferred that the flake is built up as a result of the collision and subsequent coagulation of individual elements. An average figure for the spatial concentration of ice crystals in a normal snowfall is 10/litre, their terminal velocities ranging from about 20 to 100 cm sec^{-1} in still air. In addition, of course, snow crystals generally possess considerable relative horizontal velocities which may be very important in the coagulation process as indicated by the formation of very large flakes several cm in diameter, apparently by the sideways contact of stellar crystals. Coagulation is sometimes effected by the interlocking of the arms of the component stars and is greatly facilitated if the surfaces of the crystals are wet.

Large snowflakes are thus to be expected when dendritic crystals are formed in " warm " clouds of high vapour density (temperature above about —15°C) and fall into warmer regions where coagulation occurs. In cold clouds of small water content individual crystals will largely predominate.

Conclusion

In this paper an attempt has been made to discuss briefly some of the more important physical processes which appear fundamental to a study of ice crystal formation in the atmosphere. Only the simple crystal forms have been considered, but the more complex crystals consist, in general, of a combination of these basic types, transition from one mode of growth to another being imposed by changes of environment in the path of the crystal. Ice crystals and snowflakes appear to react very strongly to changing conditions in the cloud and present a faithful if complicated record of them. An understanding of how the growth form of the crystals is influenced by the environmental conditions should enable us to deduce a good deal about the scale and magnitude of spatial and temporal changes in the parent cloud.

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Some remarks on the study of the microphysics of natural clouds

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SUMMARY

The instability of cloud droplets throws doubt on some of the methods used for their measurement. In particular, the capture of droplets on fine threads yields reliable results only if the droplets are photographed immediately after their capture. Thisre quires a microcinematograph technique which has also been applied to the study of the freezing of supercooled droplets and of the presence of ice crystals in supercooled clouds.

I have already had the honour to present to this Society (Dessens 1949) an account of the application of fine spiders' threads to the study of mist particles. Although cloud droplets are very much larger and therefore more easily measured, their instability causes certain difficulties. On the one hand, neighbouring droplets may coalesce after capture, thus increasing the apparent number of large droplets ; on the other hand, single droplets show a notable tendency to evaporate after capture on the threads, even though the whole apparatus is in thermal equilibrium with the air in the cloud. Although coalescence may be reduced by limiting the volume of air examined, no means have been discovered of preventing evaporation, which may perhaps be due to deformation of the droplets by contact with the thread, causing local increase in surface curvature and thus destroying their equilibrium with the surrounding air.

It is thus clear that in order to obtain a true picture of the size distribution of cloud droplets, they must be photographed immediately after capture. This demands a cinematographic technique. The cloudy air is drawn through a circular aperture of diameter 0·5 mm, across which runs a grid of spider's threads which are finer, more regular and cleaner than those of plexiglas. A microscope objective (magnification 30x to 50x) throws an image of the threads on to a 35 mm *ciné* film in a camera whose speed can be controlled. As each frame is examined measurements are made of the diameters of only those droplets which do not appear on preceding frames. The analysis of droplet samples taken from about thirty clouds will shortly appear in the *Annales de Geophysique*. The photographs shown to the Society on this occasion illustrate the following points :

(1) *The size distribution of the droplets.* The threads are separated sufficiently for the simple law (collection efficiency proportional to droplet radius) to be applied ; the validity of this law has already been confirmed (Dessens 1947) in the case of mist particles. The necessity for this correction arises from the fact that it is not possible to make the mesh of spiders' threads sufficiently close to capture all the cloud droplets which pass through it. The results obtained so far confirm in general those of Bricard (1940) : a single frequency maximum at a radius of 4-8 microns.

(2) *The coalescence of droplets.* Droplets brought into contact on the threads always coalesce, at least whenever their radii exceed one or two microns.

(3) *Very small droplets and the fine structure of clouds.* Some experiments are in progress to detect the presence in clouds of a class of very small droplets (of radius some tenths of a micron) which would explain the observed increase of opacity in certain clouds towards the shorter wavelengths, but no such small droplets have yet been found. On the other hand, observations, at least those made on mountain tops, reveal considerable inhomogeneities in certain clouds, both in droplet concentration and in the size distribution.

(4) *The detection of ice particles in supercooled clouds.* The aspiration of cloudy air results in a concentration towards the centre of the grid of threads of the supercooled droplets which are always preponderant in the range of temperatures studied so far (0° to —16° C). Neither the shock of collision with the threads nor the bombardment by later arrivals of the large drops formed by coalescence ever induce freezing. But freezing occurs immediately an ice particle is captured. This provides us with a method of detection of ice particles among the very much more numerous cloud droplets. Up to the present, ice particles have been detected in about 50 per cent of clouds examined between 0° C and —12° C, and in 100 per cent of clouds below —12° C. However, the number of results is still small (16) and, in any case, measurements at mountain summits must often be treated with some reserve owing to the presence of snow-covered ground in the neighbourhood.

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Electrical coalescence of fog and clouds

By P. QUENEY for M. PAUTHENIER
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Professor Pauthenier's work concerns the electrical coalescence in fogs and clouds by means of charged particles projected above them. Two preliminary papers were published by the author in the " *Comptes-Rendus de l'Académie des Sciences de Paris* " :

(i) On the electrical sweeping of fogs (1947, **224**, p. 142).
(ii) On the electrical coalescence in fogs and clouds with a positive temperature, by means of charged aerosols ; first experiments (1948, **226**, p. 587).

In a later paper yet unpublished, Pauthenier and Loutfoullah give the complete theory of electrical sweeping, assuming no turbulence. The following symbols being used :

O = charged spherical particle ; P = neutral droplet of fog (or cloud) ; q_0 = charge of O ; R, r = radii of O and P, respectively ; μ, ω = densities of O and P, respectively ; g = acceleration of gravity, the theoretical discussion shows that, in order that P shall coalesce with O as a result of electrostatic attraction, the initial distance from P to the approximately vertical path followed by O must be smaller than the critical value d given by

$$d = \sqrt[5]{\{45\, q_0^2\, r^2/16\, g\, (\mu R^2 - \omega r^2)\}^{1/5}}$$

For a greater initial distance, the droplets trajectories are only deflected towards that of the charged particle.

The "gain" obtained by using a charged particle instead of a neutral one is thus

$$\gamma = d^2/R^2$$

For instance, if $R = 10^{-3}$ cm ; $r = 0{\cdot}5 \times 10^{-3}$ cm ; $\mu = \omega = 1$ g cm^{-3} ; $q_0 = 4 \times 10^{-4}$ e.s.u. (latest experiments by Demon), one gets

$$d = 11 \times 10^{-3} \text{ cm} = 11\, R, \text{ whence } \gamma = 120.$$

CONCLUSIONS

The theory proves that, in order to precipitate out a normal fog by seeding it with neutral water droplets of radius $R = 10^{-3}$ cm, one must use at least the amount

$$M = 660 \text{ kg of water per } 10^4 \text{ m}^2,$$

while the same result is obtained with only

$$M = 6 \text{ kg of water per } 10^4 \text{ m}^2$$

if charged droplets are used.

It is to be noted that such an application is possible thanks to the fact that the disruptive electrical field of small droplets is very large (otherwise there would be a discharge between O and P with the very strong charges used).

It is also to be noted that, in the case of a thick fog, it is necessary to take into account the fact that the radius R of the charged droplet increases as it falls through the fog.

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On the freezing of droplets of water and of aqueous solutions

By C. LAFARGUE
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INTRODUCTION

The use of ultramicroscope techniques, permitting the detection of ice crystals in supercooled water clouds, has yielded much information on the degree to which clouds of water droplets formed on natural or artificial condensation nuclei may be supercooled. The freezing point of such small droplets appears to be near −41°C, although certain nuclei seem able to provoke freezing at higher temperatures.

We decided to study the freezing of individual droplets of pure water and of aqueous solutions, both in air and in other media, using the techniques described below.

1. COOLING IN AIR

The droplets, placed on a fine spider's thread of diameter 0·1 to 0·5 μ, were observed by microscope with direct illumination. The spider's thread itself was stretched across the exit of a copper spiral, of internal diameter 0·3 mm and length 50 cm, immersed in a cooling bath. A slow current of air passing through the tube ensured that the droplets were kept at the same temperature as the bath and in air saturated with respect to ice at that temperature. Measurement of the diameter of the droplets at a temperature between 0°C and —10°C gave, by Raoult's Law, the concentration of solute in each; thence the concentration at any other temperature could be obtained by a simple measurement of diameter.

Droplets of diameter 1 to 10μ of solutions of $NaCl$, KI, $MgCl_2$ and ZnI_2, cooled in air cleaned by passage through cotton-wool filters, all showed the same freezing point: —40·5° ± 1·5°C. These salts were chosen for experiment on account of the wide variation in their hygroscopicity. It was necessary to use filtered air for cooling in order to avoid contamination of the droplets or of the threads themselves by atmospheric nuclei.

2. COOLING IN THE ABSENCE OF AIR

The droplets were suspended on a grid of spiders' threads mounted on an iron-nickel thermocouple in the form of a ring of diameter 0·5 mm, which was immersed in a cooling bath of paraffin oil. Experiment had previously shown that the solubility of water in this oil was negligible at temperatures below + 10°C. This method allowed the study of the freezing of droplets of solutions whose concentration was independent of temperature. Two difficulties, however, were encountered:

(a) the increase in viscosity and opacity of the oil at low temperatures,
(b) the misting of the surface of the oil exposed to the air.

The former difficulty was overcome by replacing the paraffin oil by a "silicone" whose viscosity (350 centistokes at 25°C) varies little with temperature and whose density (0·97), freezing point (—52°C) and total immiscibility with water render it particularly suitable for this purpose; the latter difficulty by immersing the objective in the liquid.

Another technique was to replace the spiders' threads by a mercury surface on which the droplets rested. The temperature of the droplets followed that of the mercury, which was measured by thermocouple, and the freezing point of the mercury at —39°C provided a useful fixed point.

The results obtained by these two methods were identical. Droplets of diameter 1 to 20 μ, whether of distilled water or of dilute, concentrated, or even supersaturated solutions of $NaCl$, KI, $MgCl_2$ or $ZnCl_2$, froze at —40·5° ± 1·5°C. It seems, however, that the freezing point of large droplets is influenced by their size and concentration and can be much higher. In this case the observations show considerable scatter and a large number of measurements will probably be necessary to determine the relation between the freezing point and other factors.

The frozen droplets remain roughly spherical, sometimes perfectly transparent but more often opaque and consisting of a mass of small crystals of ice and of the solute. The freezing of the small droplets (1 to 10 μ) was quasi-instantaneous, taking certainly less than 1/10 of a second.

CONCLUSION

Droplets of radius 1 to 10 μ, either of water or of aqueous solutions of various concentrations, freeze at about —41°C both in air and in other media. Surface tension does not therefore seem to be an important factor and the cooling of droplets by immersion in oil or " silicone " appears justified. This method is extremely convenient and is especially suitable for the study of cloud and rain droplets. The freezing of droplets at —41°C seems to be a molecular property of water itself, unless one admits the possibility of the activity in all experiments so far of some freezing nucleus which is effective at this temperature.

Discussion

Mr. H. P. Palmer (*Clarendon Laboratory, Oxford*) : Dr. Schaefer has referred to the fragmentation process, or splintering process as we prefer to call it, by which ice crystals can reproduce themselves. This process was first observed by Findeison, and has been further investigated in some preliminary work here at the Clarendon Laboratory, Oxford.

There are two ways in which splintering can occur and which are perhaps important in the atmosphere. These are

(1) By the ejection of small splinters of ice as water freezes and

(2) the production of small splinters of ice from growing hoar frost.

In clouds the first will be important during hail formation or in any kind of riming, and the second in the growth of snow flakes. Our investigations have shown that both effects are very variable in their operation.

The splintering which occurs during riming has been investigated by moving a fine wire through a supercooled cloud in a large expansion chamber so that riming occurs upon it. Ice crystals can be seen falling down below the wire from which they must have originated. At present it seems likely that the splinters are quite small and we see them only after they have grown in the supersaturated conditions.

The splintering which occurs during the growth of a hoar-frost deposit has been attacked by passing air of controlled temperature and water content over a cooled surface, the temperature of which could also be regulated. The surface was in the upper part of a large expansion chamber, which, after an expansion, contained supercooled cloud. Splinters formed during the growth of a hoar frost deposit on the cooled surface, grew in the supercooled cloud and could be seen falling through the chamber. During the growth of the deposit the surface was also watched through a low-power microscope. The following principal results were obtained :

(1) If the conditions are such that the crystals form in *solid* hexagonal prisms, no splintering occurs.

(2) If the crystal growth is dendritic, that is to say the hoar-frost grows as fine needles, splintering occurs. Also needles, which normally stick up, so that the deposit looks like a pine forest, can occasionally be seen to fall down under the ventilating air flow, though none have been seen to fall away. It seems possible that the splinters which fall away are very small fragments formed at the time of breaking.

Snow crystals do of course frequently contain very fine structures which could break easily and broken flakes are often found ; on the other hand the solid hexagonal plates often found in Ci. clouds would probably grow without any splintering at all.

We have also observed how ice grows very fine dendritic crystals when growth occurs under isothermal conditions, in air roughly saturated with respect to water. We have taken a small plate, usually with a polished gold surface and in clean conditions grown upon it a heavy dew deposit so that the plate becomes covered with drops of various size up to about 1/2 mm diameter. When the plate is cooled below the freezing point, the larger drops usually freeze when the temperature has fallen to about —20° C and the smaller drops at lower temperatures. While the plate is above this temperature the drops remain as supercooled water. Once freezing begins in a particular drop, the drop goes white very quickly as dendritic ice crystals spread through the drop. We have shown by a *ciné* film of the freezing that crystals spread through a 1/2 mm drop is less than 0·01 second. If the plate is left with, say, one drop frozen, then very fine, dendritic crystals begin to grow on the frozen drop towards the water drops. Eventually the fine dendrites grow to touch the neighbouring water drop which immediately freezes. Mr. Ludlam has drawn our attention to the fact that this phenomenon may be used to get an idea of the rate of growth of ice crystals in a supercooled cloud. The dendrites we have observed are very fine, not more than 5μ across and would rupture and splinter very readily.

MR. A. W. BREWER (*Clarendon Laboratory, Oxford*) : I would like, if I may, to be associated with Mr. Palmer in this work on the reproduction of crystals by splintering which has seemed to me to be a " keystone " in much of cloud physics, particularly in the physics of precipitating clouds. The effect was first described by W. Findeisen and he considered its effect in clouds in 1943. But since then the effect seems to have been quite forgotten even by German workers and in the FIAT reports splintering is only mentioned as regards the electrification effects.

We are however much troubled by the variability of the rate of splintering. Mr. Palmer has described how splintering does not occur from columnar crystals but only from dendritic crystals and both are found in the air. Similarly splintering does not occur when a clean dew drop freezes but a drop of Oxford rain splinters copiously on freezing ! This variability is probably significant in the atmosphere. Thus is a cumulonimbus cloud where relatively dirty air is being drawn up from near the ground, and the crystal growth-rate is rapid there is probably a great deal of splintering. At other times when the air is clean and crystal growth is slow there may be no significant splintering.

The effects described by Mr. Palmer of how the freezing of one drop in a dew deposit leads to the feeezing of others by the growth of fine dendrites from the ice toward the water, is of interest in considering the production of hoar frost on the ground. Since freezing nuclei are so rare it is surprising that in the morning after hoar frost there are ice crystals everywhere. It will be seen at once that by this process the freezing of one single drop can result in the freezing of nearby dew and in the morning can have affected a large area. Thus far fewer freezing nuclei are necessary to produce hoar frost than might appear at first sight.

Reference

Findeisen, W.
and Findeisen, E. 1943 *Met. Z.*, Braunschweig, p. 145.

Mr. E. W. B. Gill (*Clarendon Laboratory, Oxford*) gave a description of experiments he had done with Mr. G. F. Alfrey on the electrification produced when water droplets are blown against ice and rebound. It was found that the droplets came off with a positive charge and the ice was left negatively charged. The magnitude of the charges increased very rapidly as the temperature of the ice was reduced. The magnitude also increased if the velocity of impact was increased.

The effect is probably due to a contact potential between water and ice.

The charges produced are very considerable, for example, with an impact velocity of about 30 mi/hr the charge per cm^3 of droplets is 40 e.s.u. for ice at —30° C and 100 e.s.u. at —37° C.

The experiments are only in an early stage but there is an indication that with ice around 0° C a reversal of polarity occurs.

Dr. Frith (*Meteorological Research Flight, Meteorological Office*) agreed with Dr. Schaefer that the use of artificial seeding as a method of marking cloud and then watching to see what happened was valuable. Photographs were shown of an experiment made in this country wherein seeding had revealed an otherwise unsuspected and very sharp, horizontal shear of wind within the cloud.

Dr. Frith also showed photographs of a new type of crystal, present in relatively large numbers, even at great heights. The photographs showed a thin film of water covering more or less of the surface of the crystal as the humidity increased or decreased. The crystals appear to be insoluble in water.

Dr. J. A. Prins, (*Delft*) : We were told by Dr. Lafargue that he has undercooled water drops of 100μ diameter to —41° whereas —14° has been considered hitherto as the maximum undercooling for quantities of water or salt solutions, of some cm^3 (Hollstein). I want to point out an analogous behaviour of mercury which is not easily undercooled in bulk but may be undercooled to less than —100° when it is present as small drops of about 20μ (*Z. Phys.*, **56**, p. 617). The explanation is of course that the probability of nucleation in a small volume is small.

Regarding Dr. Schaefer's remark that most minerals can act as freezing nuclei at sufficient undercooling it may be remembered that at the pulsator tables in Kimberley all minerals prove to be more or less hygrophilic except diamond. This suggests that diamond powder would act as little as a seeder as paraffin would. It would not be very expensive to try this out.

The experiments of Dr. Gill on the electrification of surfaces by a jet of water are in accordance with Cohn's rule and so are explained easily as contact electricity. References on this subject may also be found in Dr. C. Kramer, *Med. en Verh. K.N.M.I.*, De Bilt, No. 102, 1948.

CENTENARY SYMPOSIUM

ON

STRUCTURE OF WEATHER SYSTEMS

Held at Oxford on the afternoon of 29 March 1950

Chairman : Dr. R. C. SUTCLIFFE

551.515

The structure of weather systems

By W. BLEEKER

Royal Netherlands Meteorological Institute, De Bilt[*]

1. Introduction

Speaking or writing on the subject " The structure of weather systems " it will be necessary first to discuss the meaning of the expression *weather system*, or rather of its parts *weather* and *system*.

Aeronautical meteorologists have given the word " weather " the somewhat limited sense of clouds and precipitation, linked with frontal zones, convergence zones or low pressure areas. When according to aviation jargon " no weather " is present, this generally indicates that no clouds of much vertical extent and no precipitation are observed. An analogous distinction between " weather " and " no weather " is made by seaman and mountaineer when they speak of the " weather side " of an island or of a mountain. The synoptic meteorologist considers such distinctions illogical ; according to him there is always " weather." For a synoptician not only hydrometeors but the ever-present wind- and pressure-field are also very important parts of any " *weather* system." It is along these synoptic lines that we will consider the meaning of the word " weather " when discussing " *weather* systems " though more attention will be given to " bad weather conditions " than to " fine weather conditions."

Some remarks have also to be made on the word " system." The main point is : what are the dimensions of the " weather *systems* " which we are going to consider ? For example, cumulus and a cumulonimbus are as much weather systems as are the tropical hurricane, the cyclone of temperate latitudes or the tropical low-pressure belt ; their horizontal dimensions however are completely different. The question arises whether such systems have something in common and whether it will be possible to treat their structure and development from a general point of view. It is the intention in this paper to make an attempt to do so. For this purpose however we have to realize that the atmosphere in building up a " weather system " very often acts as some sort of a thermodynamical machine, driven by differential heating or cooling, and that the "weather system" dies out when differential heating or cooling stops. We will first deal generally with such thermal circulations.

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[*] Many of the ideas described in this paper were developed while the author was studying the structure of weather systems as a part of a project on the general circulation at the University of Chicago under the sponsorship of the Office of Naval Research.

2. THE FORMATION OF THERMAL LOWS AND HIGHS ON A NON-ROTATING EARTH

Thermal lows and thermal highs have long been considered as weather systems built up by differential heating or cooling. Their formation has been described in many meteorological textbooks ; important parts of the theoretical problems related to these weather systems have been dealt with by Wexler (1937) and Schmidt (1946). For our considerations we need to go further than the textbooks. Let us see what will happen when on a non-rotating earth in an atmosphere originally showing no horizontal temperature and pressure differences, a broad strip of air is heated from below. The " heating function," indicating as a function of space the amount of heat which is supplied to the air at a certain moment per unit time and unit volume, may be such that there is a maximum of heating along the central line ; there is no heating outside the boundaries of the strip. The usual way of reasoning on the result of this differential heating goes as follows. The heated air expands and a lifting of the isobaric surfaces will gradually occur through an increasing depth. The resulting higher pressure present in the higher layers makes the air move laterally away from the heated centre, causing a pressure fall in the lower layers over the centre, where the air will move in laterally. A circulation system develops, whereby the air for continuity reasons rises in the heated centre and descends in the environment.

It is important to emphasize some points which are not generally mentioned in meteorological literature.

(a) *The importance of friction*

It is, as far as is known, seldom realized that if the motion were frictionless, a pressure field of less importance would develop. The heat supplied would be converted into internal energy of the gas and an ever increasing kinetic energy of the moving air. However, since there are frictional influences, the diverging air in the higher layers will be subject to less frictional drag than the converging air in the lower layers. One can consequently expect that during a period wherein heating increases with time a substantial pressure-field will build up since the outflow in the higher layers will be larger than the inflow in the lower layers. Heat is now converted into the internal energy of the gas, into kinetic energy which is partly converted into frictional warmth, and into the potential energy of the pressure field. A certain quasi-constant circulation may be attained when the heat supply no longer increases with time. When the heating stops the potential energy of the pressure field is converted into frictional warmth. The pressure field then gradually flattens out.

(b) *The form of the pressure field*

It is not generally realized that the pressure field which develops as a result of this warming not only shows a " low " over the centre of the strip, but that as a reaction and owing to the continuity of mass, two strips of high pressure will form at the borders of the heated strip. This is understandable, since the structure of the pressure field in the higher layers is such that going from the centre to the border the speed of the lateral outward motion first increases with increasing distance from the centre and then decreases with increasing distance from the centre. There will therefore be in the higher layers a mass divergence near the centre and a mass convergence near the borders, which results in a fall of pressure in the centre and a rise near the border. Fig. 1 gives the resulting pressure field which, as has been described by Bleeker and Andre (1950), will resemble the second derivative of the heating function.

The circulation system is not closed ; air leaks out of it, whereby pressure differences will be flattened out and pressure will gradually rise also outside the heated strip.

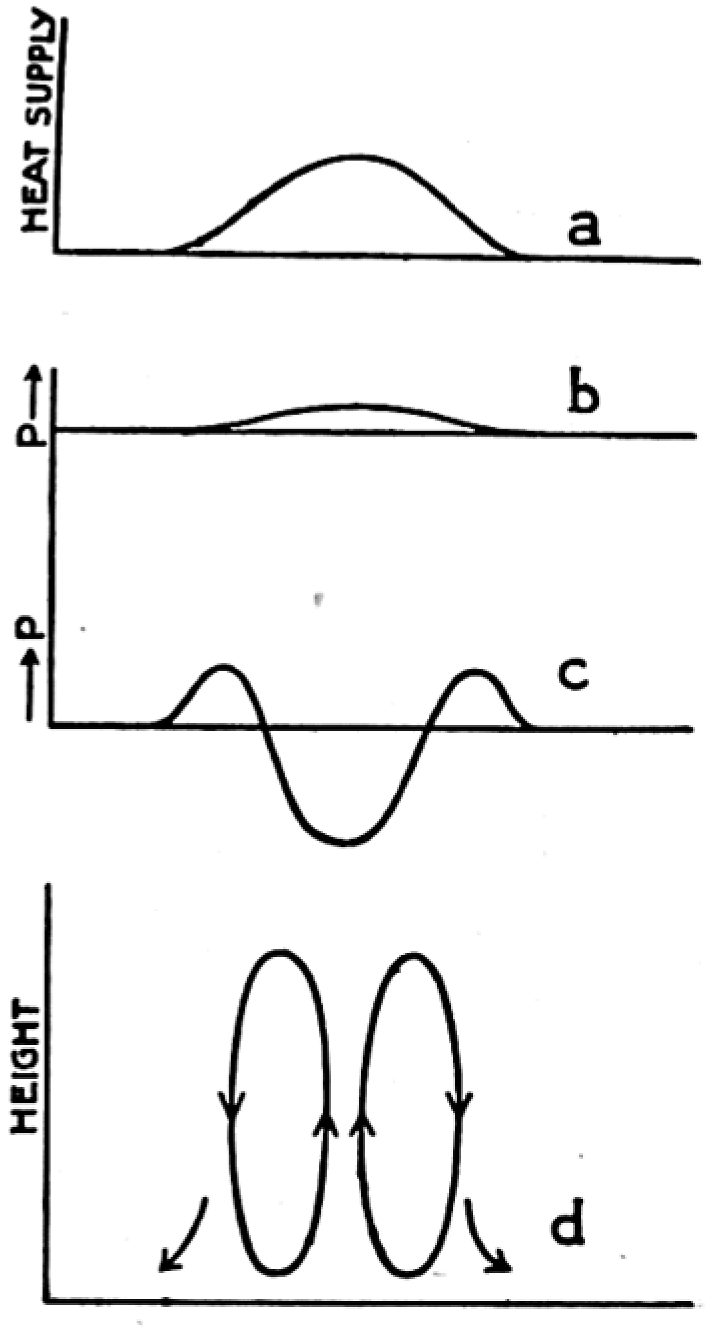

Figure 1. (a), " Heating function ", (b and c), pressure field in higher and lower layers and (d), circulation-system when a strip is heated.

(c) *Direct and indirect circulation*

It is logical to expect that a circulation system like this must, when the warming goes on and it is sufficiently strong, gradually build up from lower to higher layers. We should however be aware of the fact that not only the lower heated air takes part in the circulation. The dilatation of the lower layers of course causes also a rising of the isobaric surfaces in non-heated air which will also move laterally away from the centre, so that lower air will rise, which is cooled adiabatically and becomes colder than its environment in which we have a descending motion with relatively high temperatures as a result of adiabatic heating (at least when we suppose that the atmosphere in the higher layers has a stable lapse rate). If we call the lower part of the circulation system, where the temperatures in the centre are higher than those of the environment, thermally *direct*, we can give to the upper part of the circulation the name thermally *indirect*. The indirect part of the circulation cannot exist without the direct part.

(d) *The release of latent heat*

It is of importance to state that a release of latent heat in the rising currents by the formation of clouds and precipitation means an extra warming. The trough of low pressure over the centre and the ridges of high pressure at the borders of our strip will then be more pronounced.

3. THERMAL LOWS AND HIGHS ON A ROTATING EARTH

If we transform our picture from a non-rotating earth to that on a plane rotating with constant speed, nothing essential will change, except for the fact that the air will not move perpendicular to the isobars but more or less parallel. Here a point should be emphasized which is often overlooked. One is used to wind arrows which make a certain small angle with the isobars on surface maps. It is generally forgotten however that in the higher layers also the wind arrows must make a (be it then) very small angle with the isobars or contour-lines. There *must* always be frictional influences also in the higher layers. That a very small angle between wind and isobar (or contour line) direction must exist in the higher layers becomes clear also when we think of a stationary situation in which the pressure field (for instance of our strip) is neither deepening nor filling. We then have convergence at the surface, but the stationary situation could not exist if in the higher layers the air were moving along the isobars or contour lines.

It has been said at the beginning of this section that nothing essential in our reasoning will change when considering a rotating instead of a non-rotating plane. This certainly is the case when we are dealing with the particular situation of the heated strip from which we started. In reality however the atmosphere offers us more complicated situations. Whereas on a non-rotating earth the surface air will move perpendicular to the " lines of equal heating " towards higher or lower values (higher or lower temperature), and whereas in our particular example on a rotating earth it can be expected that the streamlines always show a movement (not now, however, perpendicular to the lines of equal heating) either to higher or to lower temperatures, this will not normally be the case in nature. Suppose for instance that a closed low forms at a certain location in our strip as a result of the release of latent heat, then the air circling around the low will be warmed when moving towards the centre of the strip and be cooled when moving towards the border. The thermal exchange between the surface and the air becomes complicated and this will again influence the pressure field. Furthermore, one has to be aware of the fact that on a real earth the deflecting force of the earth's rotation varies with latitude. The above remarks are made in order to show that although one may consider thermal processes as being of the utmost importance for the development and structure of weather systems, one has to be aware of all inertia forces connected with moving air particles. Whereas synopticians who favour the idea that heating and cooling play an important role in meteorological processes are always aware of these forces, past and recent trends in the development of theoretical meteorology have often been to neglect the thermodynamical processes and the friction owing to mathematical difficulties. As long as this happens we feel that dynamic meteorology can only be of little help to synoptic meteorology.

4. EXAMPLES OF THERMAL WEATHER SYSTEMS

A striking example of the weather systems described above is the tropical low-pressure belt. This is a heat-low of enormous dimensions where great amounts of latent heat are released. The indirect part of the circulation expresses itself clearly by the great height of the tropopause and the low temperatures in stratosphere and upper troposphere. And in spite of all that has been written on the dynamic origin of the subtropical high pressure areas, it seems worth while, or at least very tempting, to return to a revised form of Hadley's ideas on their formation, and consider them as the " reaction " ridges of high pressure in our example discussed in section 2. The big areas of low pressure in summer and of high pressure in winter over the continents are other well known examples of thermal weather systems. Heat-lows of smaller dimensions are the winter lows in the Mediterranean, over the Black Sea, the Great Lakes in America, and the summer monsoon low over India. One may ask why such lows which are clearly shown in climatological mean maps are not always present over these areas during the favourable seasons. One of the reasons is that just because the earth rotates (*cf.* section 3) such areas can be filled from other sources with air which, instead of being warmed, is cooled. Another reason is that the air in the environment of the place where normally a low is found is temporarily heated even more, for instance by the release of latent heat. The importance of the " character " of the air which takes part in the circulation of a weather system on the behaviour of that system can be derived from the following facts. A Genoa low or a Black Sea low deepens when fresh air is transported from the north ; the Indian monsoon low fills when drier, warmer air from the west or north-west is caught in its circulation. Furthermore a high builds up when in the higher layers advection of warmer air occurs ; it breaks down when cold air is advected in the upper layers. The tropical hurricane may be considered as an example of a moving heat-low in which release of latent heat plays the most important role. A moving heat-low of small dimensions is the cumulus and a cold cell of small dimensions is the thunderstorm high as has been shown by Bleeker and Andre (1950).

5. QUASI-STATIONARY " LINEAR " WEATHER SYSTEMS

We will now discuss (see also Bleeker 1949) the situation in which the heating function has approximately the form of the hyperbolic tangent (Fig. 2). For example this case occurs along a coast line when the air over land is warmed rather uniformly and is cooled over the sea ; it occurs also along the borders of snow and ice fields or along the borders of warm and cold ocean currents. Let us start again with an atmosphere originally showing no horizontal temperature and pressure differences and discuss what will happen. It is not difficult to see how the pressure field will change in the higher layers ; the uniform rising of the isobaric surfaces over the heated half and the uniform lowering of these surfaces over the cooled half will cause in the higher layers a narrow band over the border where the pressure gradient is high. Here on a rotating earth the jet stream will develop. Even in a stationary situation upper-air divergence will be present where there is anticyclonic shear, convergence will exist where there is cyclonic shear, (recalling the discussions of para. 3 on the frictional influences in the higher layers). Owing to the divergence over the anticyclonic shear area in the higher layers a surface trough of low pressure will develop over the warm side of the border and owing to the convergence over the cyclonic shear area in the higher layers a surface ridge of high pressure will develop on the cold side of the border (Fig. 2). The surface pressure field resembles the second derivative of the heating function.

Again a circulation system forms ; the air will rise in the trough, descend in the ridge. The line of convergence is nothing else than a frontal convergence zone. Notice that the frontal convergence and wind shift are situated over the warm area, and that warm air, and the cold air which is heated over the sea, meet

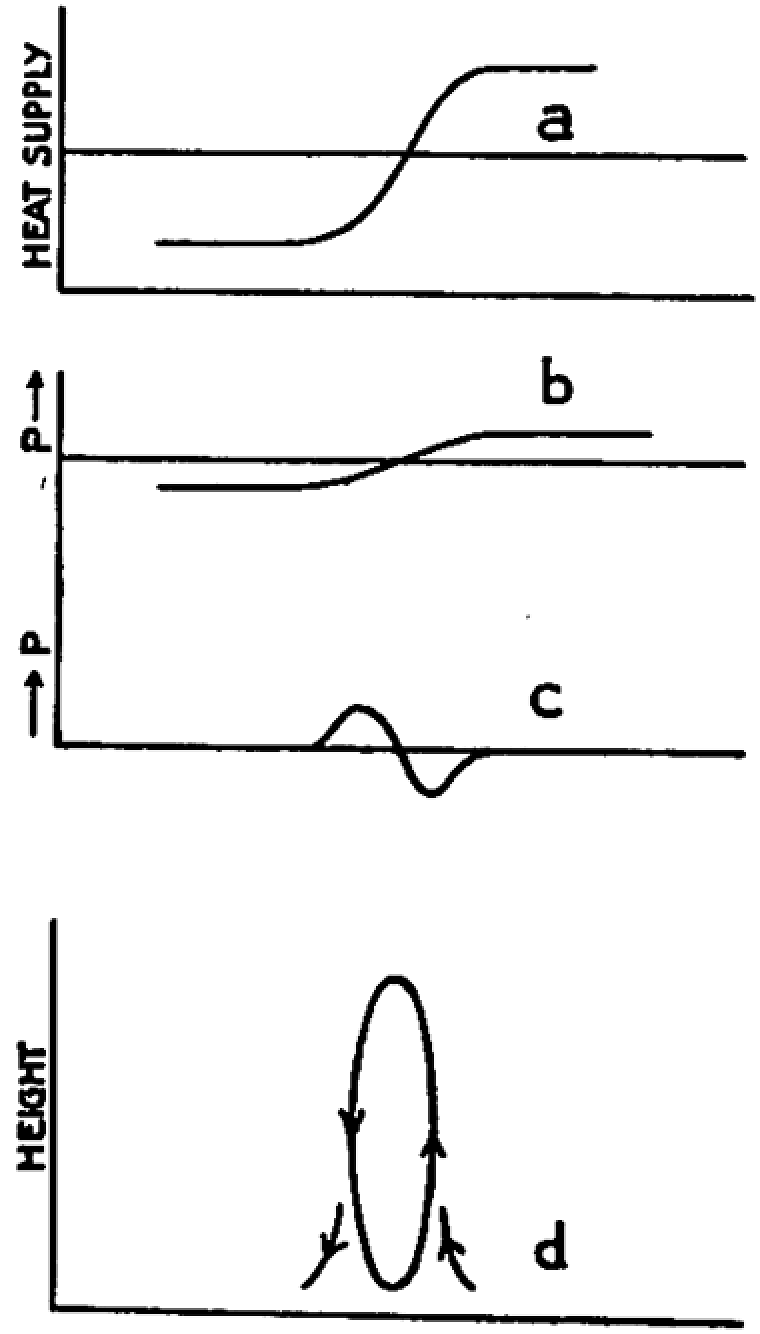

Figure 2. (a), "Heating function", (b and c), pressure field in higher and lower layers and (d), circulation-system, when differential heating is limited to a line (uniform heating and cooling on either side of the line).

each other here. The greatest temperature discontinuity will therefore not be present in the centre of the trough, but at the border of the heated and cooled area, as is actually observed on weather maps. The circulation system which we meet here is again not closed. From the heated part air will move into the trough; from the ridge air will move into the cooled part. Again here ascent and descent of isobaric surfaces will also take place in the non-heated and non-cooled air of the higher layers, which therefore will also take part in the circulation. If the circulation system reaches high enough we shall find an upward bulge in the tropopause with low stratospheric temperatures over the warm trough; there will exist a downward bulge in the tropopause with high stratospheric temperatures over the cold ridge.

When, as is usually the case, clouds develop in the ascending part of the circulation system, the release of latent heat changes the form of the heating function into that of Fig. 3a. The resultant pressure field, which again resembles the second derivative of the heating function, will show also a small ridge of high pressure over the warm area (Fig. 3b). The ascending motion splits in two directions and the frontal cloud system will have a toadstool form (Fig. 3d), so well known from pictures of cold-front clouds in textbooks. It is clear from the above that stationary jet-front systems will only develop in the manner described when the temperature

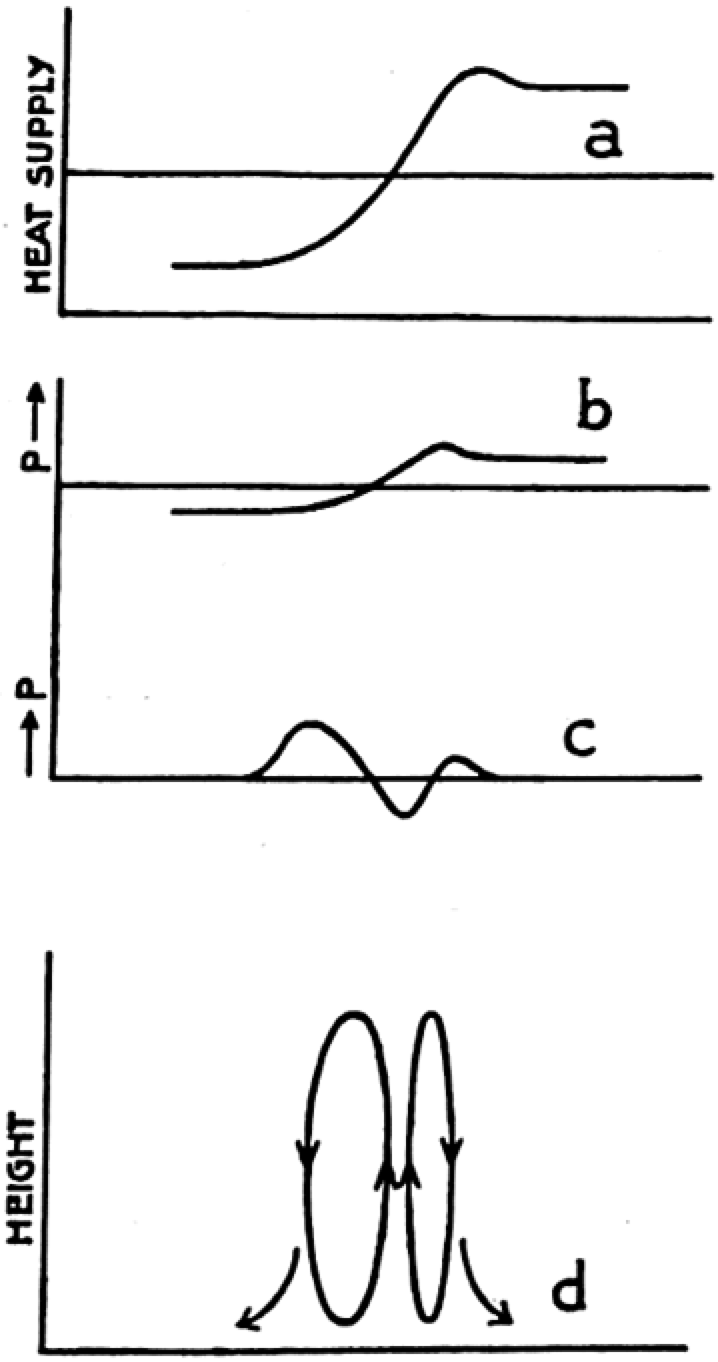

Figure 3. (a), "Heating function", (b and c), pressure field in higher and lower layers, when in the case of Fig. 2 latent heat is also released.

differences are great enough, and exist for a sufficiently long time. Jet-front systems of this kind are often found in winter along the east and south coasts of U.S.A. and along the east coast of Asia. In summer they may be present along the north coast of Africa. Coastal fronts formed as a result of differential heating within a rather short time have also been described by Australian meteorologists.

The above gives a description of the formation of a frontal jet stream system along a line of differential heating (physical frontogenesis). It has to be mentioned that kinematical frontogenesis is also possible and will lead to the formation of the same circulation system. Such frontogenesis will be described by Nyberg (1950); it takes place when a current of warm air is brought into contact with a colder current from another region as sometimes occurs in the northern hemisphere on the south side of a low near or east of Iceland, when a cold northerly arctic-air current bends towards south-east or east.

Weaker systems than those described above and formed by differential heating will be present as land- and sea-breeze circulations along the coast. These circulation systems, which do not differ in principle from the jet-front systems have too little time to build up to greater heights, and to give the deflecting force of the earth's rotation opportunity to gain control of the motion.

We conclude our remarks on the stationary quasi-linear weather systems by asking what will happen when differential heating ends, for instance if we slowly bring the temperatures on both sides of the line of differential heating to the same value. It is evident that the circulation system keeps going for some time. But in the long run all differences in pressure will be flattened out.

6. Moving "linear" weather systems

In the foregoing paragraph we discussed the formation of a front (physical frontogenesis) along a line where we find a concentration of earth- or sea-surface isotherms. This leads to the formation of a quasi-stationary front; the front is not completely stationary since the trough of low pressure (*i.e.* the line of convergence) moves a little during the building up of the circulation system. This small movement is due to the increase of heating and/or cooling and probably also to the tendency for a flattening out of the pressure field. We know that this movement really exists since a sea-breeze front, which is also nothing more than such a convergence line, also moves inland in the course of the day. However, such quasi-stationary fronts and also kinematically formed fronts do not remain at their birthplace, but generally move for reasons to be discussed later. Let us, for instance, consider a cold front, which, we know by experience, travels approximately with the geostrophic windspeed of the lower layers. What is the mechanism which is responsible for the movement of the frontal convergence line (the frontal trough) and the related jet stream? We touch here upon an interesting question which has been discussed by Sutcliffe (1938). Basing his statement on the well known paradox of Jeffreys, Sutcliffe points out that it is a wrong conception to think that the frontal trough moves simply because there is an advection of cold air. If the motion is really geostrophic the "cold wall" will move but an associated frontal trough would remain at the same place. Sutcliffe also points out that in that case the pressure field in the higher layers will change with the arrival of the cold air. This now gives the answer to the problem of the motion of the front-jet stream system. When the wall of cold air, that is the rather steep zone of baroclinity, moves, the zone of strong pressure gradients, that is the jet stream, will move with it. The most important result of this displacement of the jet stream is that the anticyclonic shear zone, and thus the zone of high-level divergence, on the warm side of the jet stream will also move with the wall of cold air. As a result the surface pressure keeps falling ahead of the frontal trough. Since also the cyclonic shear zone, and thus the zone of high-level convergence, on the cold side of the jet stream moves with the wall of cold air, pressure rises in the rear of the frontal zone.

According to this idea the rather steep zone of baroclinity is the most important factor in the front-jetstream circulation system. The system keeps going as long as temperature difference, that is as long as the zone of baroclinity, exists. However, since the system is loosened from its source, it is similar to the stationary case when heating or cooling stops. The cold air, now moving over a warm surface, picks up more heat than the warm air, which is an important frontolysing factor. In the cold air a turbulence- or convection-inversion will form, which inversion is often considered as a frontal surface. This however is wrong as we may see when we consider the humidity, which is always higher under the inversion than above it. Maritime polar air under this inversion in Western Europe has a saturated adiabatic temperature gradient ; the air above the inversion has also to be considered as polar air and not as cold tropical air which is lifted, as has been done so often (see for instance Bjerknes and Palmén (1937)).

We have to revise therefore the classical Norwegian conception of the upslide motion (which, as Sutcliffe (1938) said, remained still to be adequately explained) since the mechanism of the " frontal lifting " has to be found in the rather steep zone of baroclinity, and the ageostrophic motion in the upper layers plays an important role in the frontal circulation. The so-called pre cold-front cooling and post-warm-front warming should be considered as the real fronts, and the surfaces which in many cases have been taken for the real frontal surfaces are of secondary importance. It has, however, to be added that the zone of baroclinity is certainly not always steep and may have in many cases, especially at warm fronts, a slope deviating considerably from the vertical. This, however, does not in principle change our reasoning, but the circulation system will be broader. It is interesting to point out that in such cases, which occur at fronts at a considerable distance from the frontogenetic areas, the jet stream must be present over the area of strong baroclinity in the *lower* layers, that is in the *warm* air in the upper layers.

An interesting point in relation to the above ideas about the frontal circulation may be mentioned. Forecasters know by experience that when, for instance in summer, a cold front approaches a warm continent during daytime, the frontal activity suddenly jumps up. This can be explained when it is realised that the warm air in front of the cold wall is warmed, so that the isobaric surfaces rise a little and the circulation is strengthened. After the invasion of the cold air into the continent the activity dies down again. The same occurs when such a cold front moves into a thermal trough of low pressure, situated over a tongue of warm water. The opposite is the case when a cold front approaches a cold continent in winter or a thermal ridge of high pressure. Warm fronts become less intense when they invade a continent in summer — more active when they invade it in winter.

A last remark which can be made with regard to the moving front-jet system is that the cold wall of air moves under, and rather independent of, the air in the higher layers. This means that the jet stream is formed from continuously changing air in the higher layers, which can in fact be deduced from a study of the temperatures of the jet stream air.

7. SYNOPTIC EVIDENCE OF THE ABOVE IDEAS ON THE RELATION BETWEEN FRONT AND JET STREAM

The foregoing considerations have been deduced from careful studies of several papers dealing with the problem of the jet stream. It would not be too difficult to add to this paper some new synoptic-aerological analyses giving evidence for the existence of the circulation system that is related to the front-jet stream weather system. The reader might wish to compare them with the work of earlier authors on this subject in order to check the objectivity of the author for which reason we shall rather reproduce a few of the most convincing analyses of other authors. Fig. 4 gives a reproduction of a vertical cross section published by Palmén and Nagler (1948). It shows a steep zone of baroclinity between latitudes 45° and 50°. The frontal layer in the lower layers seems a little doubtful*, but the tropopause is clearly recognizable. The jet stream is present over the zone of strongest baro-

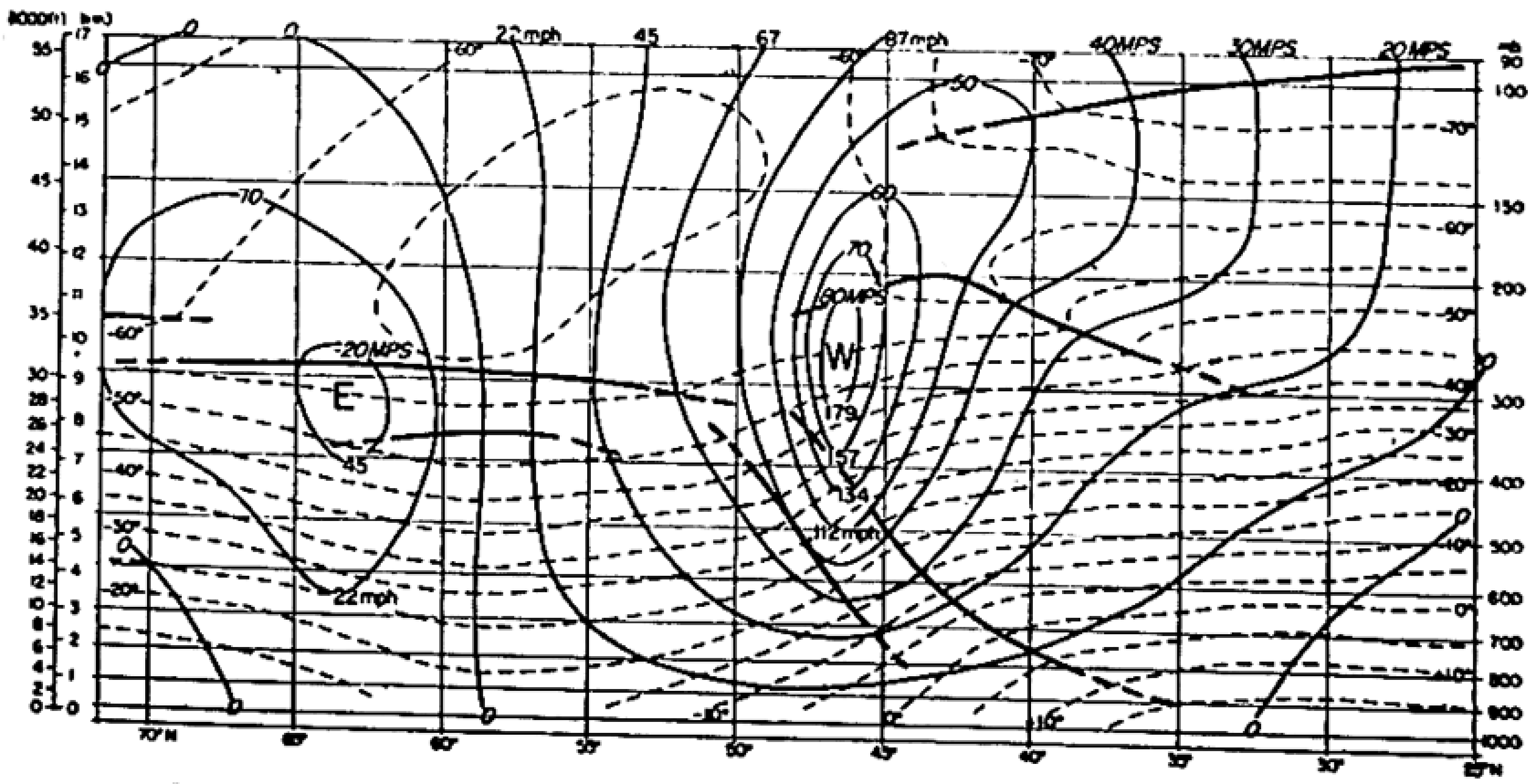

Figure 4. A mean cross-section for 0300 G.C.T. 30 November 1946, showing the average distribution of geostrophic westerly wind and temperature over North America from latitude 25° to 75° N., in a case of approximately westerly flow. Heavy lines indicate boundaries of the principal frontal layer (polar front) or tropopause surfaces. Thin solid lines indicate the velocity of the westerly component of the geostrophic wind (in m sec^{-1} and mi/hr) and dashed lines the temperature (in °C). (From Palmén and Nagler 1948.)

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* This frontal layer has even been omitted in a second analysis of the situation in the same paper

clinity in the lower layers. The lower tropopause in the warm air bulges upward at the south side of the jet stream and the stratosphere temperatures over this upward bulge are low. On the contrary the tropopause in the cold air to the north of the jet stream bends down, and the stratosphere above shows relatively high temperatures. Analogous cross sections which show clearly the same features can be found in Palmén (1948) and in Riehl (1948). Fig. 5, from Riehl, gives a surface map analysis and the location of the jet stream centre three hours earlier. It should be noted that the main front is indeed present to south of the jet centre, that is over or at least very near to the anticyclonic shear zone. Analogous indications of the presence of the anticyclonic shear zone over the main front can be found in Durst and Davis (1949).

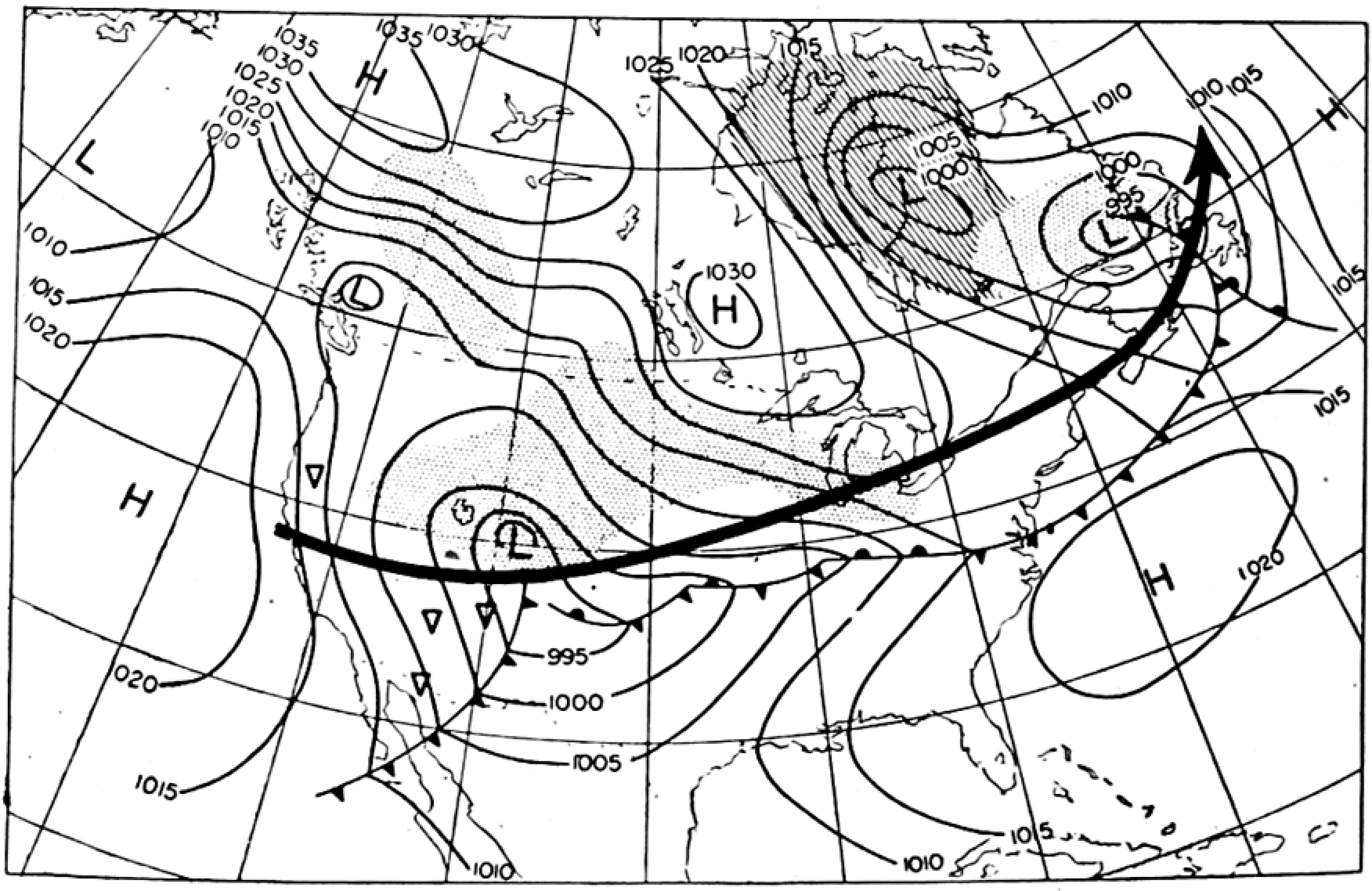

Figure 5. Surface map of 29 January 1947; heavy arrow indicates centre of upper jet stream (From Riehl 1948).

8. THE DEVELOPMENT AND STRUCTURE OF THE CYCLONES OF TEMPERATE LATITUDES

The normal cyclone or as it often is called the *wave cyclone* has also to be placed in our weather systems. How does it form and what are the reasons for its development?

Let us suppose that a front-jet stream system lies in a W.-E. direction, for instance along a snow line or along a zone of crowded sea-surface isotherms. Now several synopticians have remarked on the generation of waves which hitherto seem to have escaped the general attention of meteorologists, who have endeavoured again and again to seek the solution of the problem of the development of cyclones along theoretical lines. Mecking (1932) discussed the formation of waves and cyclones along stationary fronts between ice and ocean. He mentions the fact that waves develop when these fronts are crossed by another front. The same ideas have been expressed by Bugajew (1936) with regard to stationary fronts along the border of permanently frozen soil. Carpenter (1940) when studying occlusions on the Alaskan coast said that waves often develop along the Canadian arctic front when occlusions or cold fronts come in from the west. Conover (1941) discussed the formation of cyclones along the east coast of the U.S.A. Here again the idea has been suggested that waves often form when such a cross system coming from the west cuts the coast line.

With these ideas in mind it seems quite simple to get a picture of the development of so-called stable and unstable waves. The cross systems of the abovementioned authors are nothing more than other circulation systems, which move along the main west-east system, carried by the higher layers (700 mb or less) where we have a general west-east motion of the air. At the crossing point the systems will fortify each other ; here we find the top of the wave. The wave remains stable when the cross-system is rather weak and/or when it moves with high speed. If, however, the cross-system itself is important, then the deepening at the crossing point where the circulation systems fortify each other may be considerable and a low will form. The Coriolis force now gets hold of the air motion, with the result that the cold air breaks out over the warmer surface formerly occupied by the warm air. If the cold air moved geostrophically and if it were not warmed the pressure field would not change. However, owing to the enormous warming the pressure will fall in the cold air ; the depression centre will be displaced from the crossing point of the systems into the cold air in which the heating goes on. The fronts are now thrown cyclonically around the centre and the mature cyclone acquires the structure given in Fig. 6. Postma (1948) has already shown that the

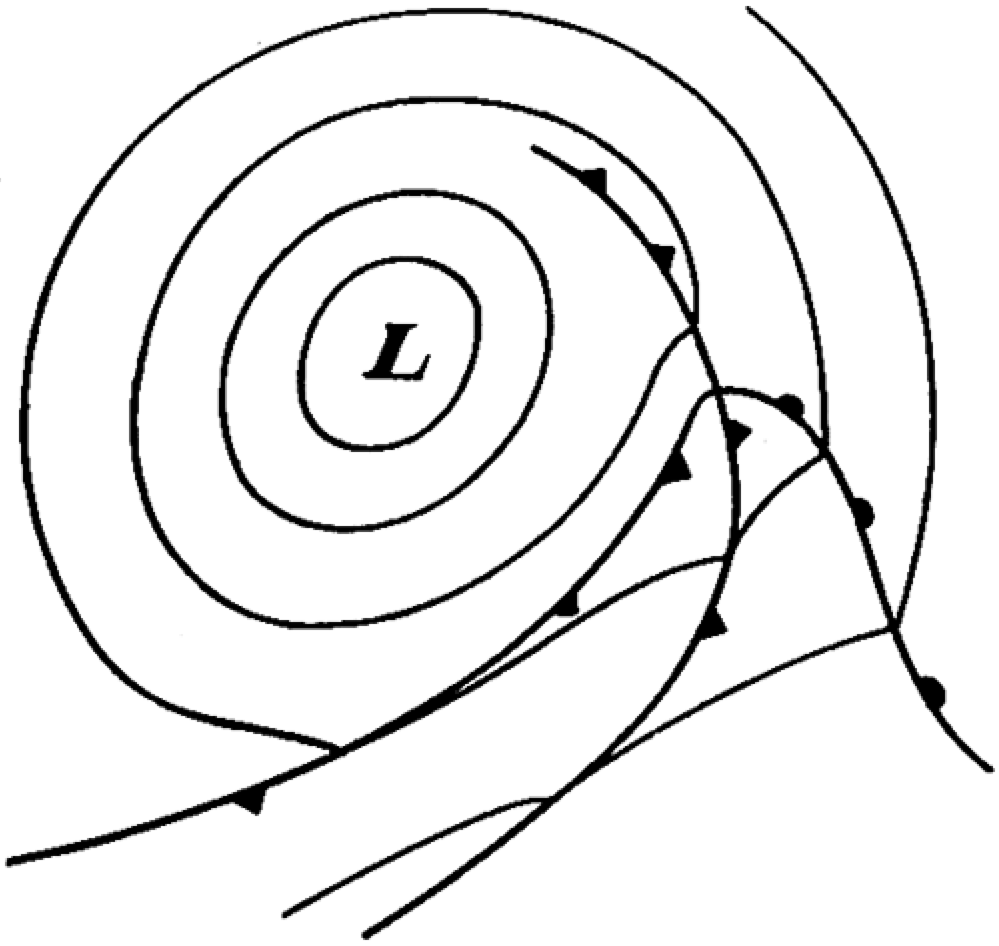

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Figure 6. Idealized structure of a mature cyclone.

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main frontal system, which up to now most authors have considered to be the only system present in a mature cyclone, is completely steered by the developing low centre in the cold air. As Postma says: there is no question of an occluding wave, at whose top the low centre develops, but the whole system starts as a frontal "sweep," which sweep in our opinion is carried by a cross system.

The part of the cross system situated in the warm air often gives the "warm-sector rain," inexplicable if we think along the lines of the Norwegian scheme. The part of the cross-system in the cold air is often, in the later stage of development, erroneously considered as the "occlusion." The cross-system in the cold air is considered by German meteorologists as an important factor in the development of a depression ("Dreimassen-Eck"). When we have two west-east main frontal systems and one cross system moves in, frontal sweeps develop along both main frontal systems, as synoptic experience also shows. The main frontal system that is thrown around the depression centre in the cold air carries the jetstream with it, and in well-developed depressions we find, in upper air charts, a tongue of warm air ahead of the cyclone, which tongue is bordered by the jetstream (Fig. 7). Here also the anticyclonic shear keeps the circulation along warm and cold front going and is responsible for the narrowing of the tongue at the surface, that is for the "occlusion." It seems as if the above can give an explanation to many questions, outlined so clearly by Boyden (1938) in an article in which he expressed or rather confessed the embarrassments which are so common to synopticians. It is at least understandable why cyclones develop preferably in situations where cold and warm air are moving quasi-parallel in the same direction; in such a situation the cross-frontal system can easily move in. It is also clear that a cyclone and not an anticyclone will develop along the main frontal system. And if we do not consider the cyclone as an unstable wave then much of the mystery of the occlusion process discussed by Brunt, in the discussion on Boyden's paper, seems to be removed.

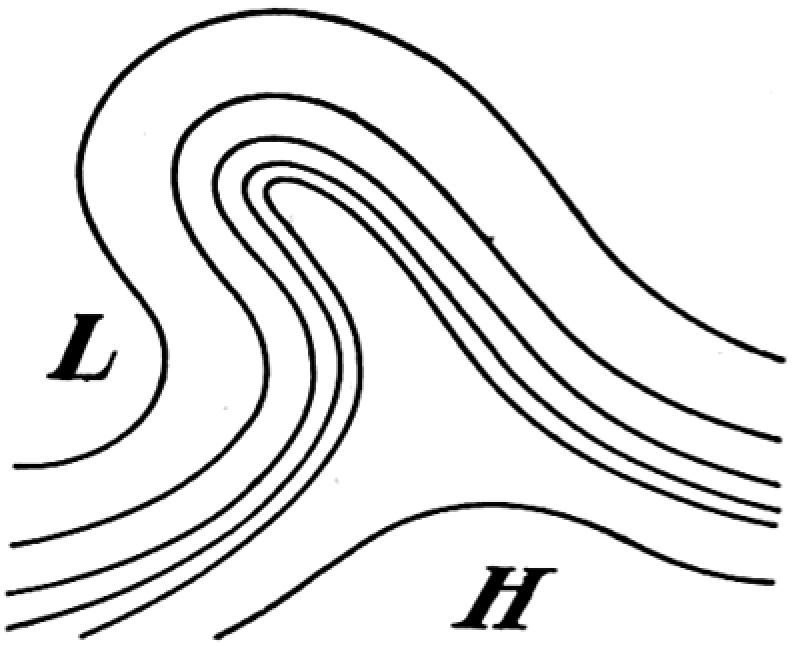

Figure 7. Idealized structure of the flow in higher layers (approx. 300 mb) in a mature cyclone.

9. Synoptic evidence of the cross-systems in developing cyclones

It is not the intention to give in this paper new analyses showing the cross system in frontal sweeps. The reader himself may consult the papers mentioned above and especially that of Carpenter. Another paper which clearly indicates the cross-system is that of Kolb and Goodmanson (1944) of which one figure, also showing the trajectory of an air particle in the rear of the depression (and therefore the warming of the air), is reproduced in Fig. 8. Another reproduction (Fig. 9)

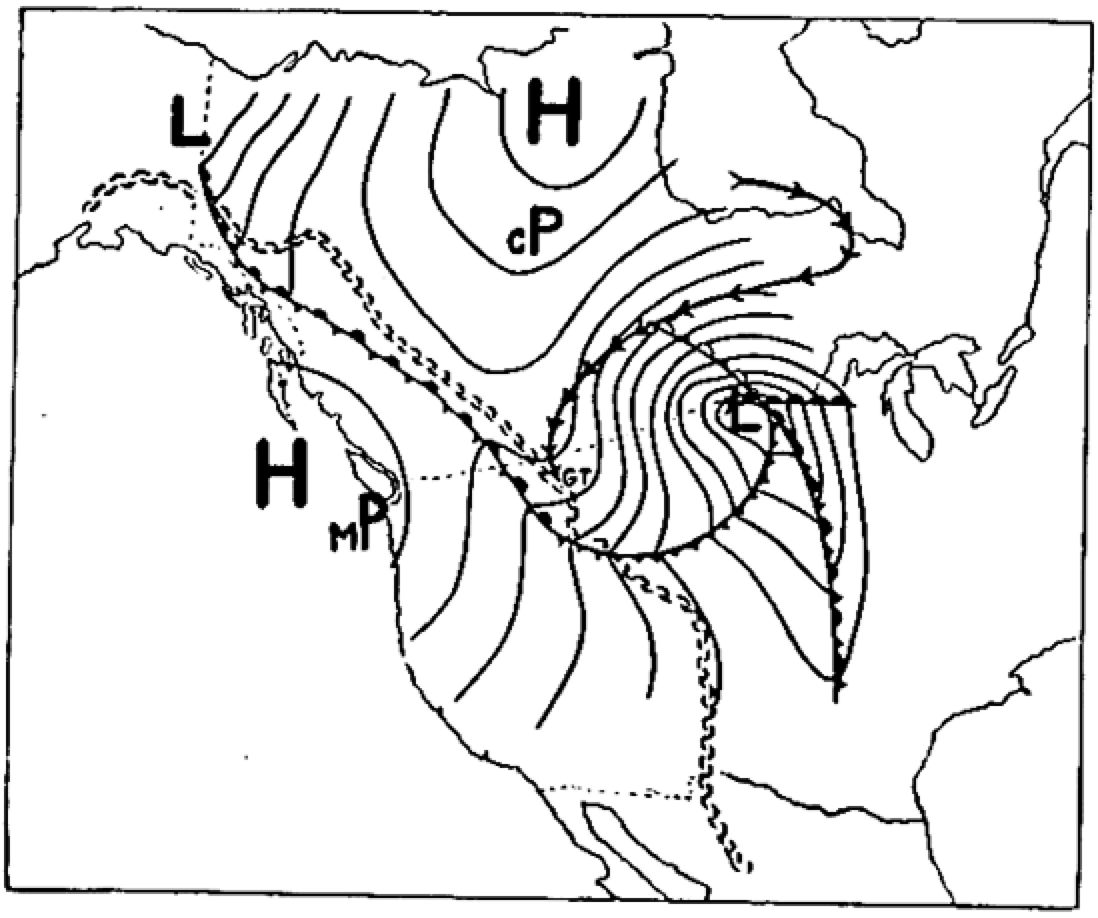

Figure 8. Surface analyses for 1830 G.C.T., 4 June 1944, showing also 78-hour trajectory of lower air (arrows are spaced at 6-hourly intervals). (From Kolb and Goodmanson 1944)

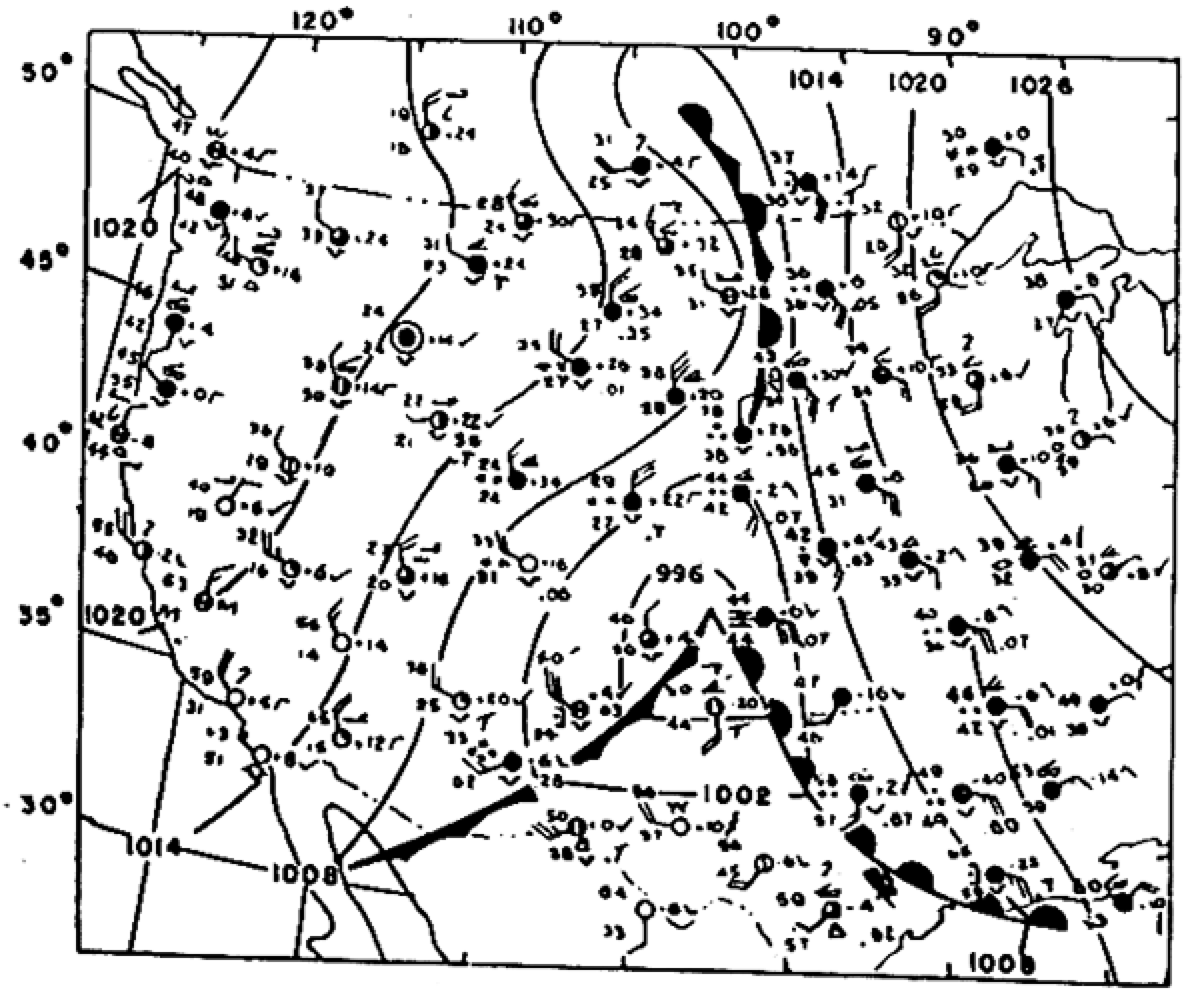

Figure 9. Surface analysis for 1130 G.C.T., 24 November 1944. (From Fleagle 1947)

from Fleagle (1947) shows a cross system which consists of the partly drawn occlusion (partly cold front ?) of an old cyclone, whose centre is found near the border of U.S.A. and Canada. It is clear that this system can be drawn through the top of the new developing frontal sweep over the southern part of the U.S.A. and that it continues even in the warm sector of this newly developing cyclone between the stations with more southerly and those with more westerly winds.

10. Concluding remarks

The aim of this paper is not only to try to give a consistent picture of our weather systems, but also to express the attitude of the author with respect to some problems in synoptic and dynamic meteorology. In a few words this attitude may be expressed as follows ; we cannot make progress in meteorology if we place pure dynamics only in the foreground of our thinking and if we do not take into consideration the fact that heat supply and heat subtraction are the most important factors in the development of weather systems. It seems moreover time for synopticians to try to find ways and means of introducing the influences of heating and cooling into daily practice and for dynamical meteorologists to attack certainly the most intriguing and difficult problem of theoretical geophysics, that of the thermal circulation on a rotating earth.

Acknowledgments

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A study of vertical motion and formation of fronts and jet streams

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SUMMARY

A synoptic study shows how the deepening of one trough is followed by the formation downstream of a frontogenetic area and later on by the formation of a jet. The vertical motion of the air on both sides of the jet is in a direct sense, *i.e.*, the cold air is sinking and the warm air ascending. Thus the front would have been much sharper but for the vertical motion. The velocity of the local wind as well as the velocity of individual particles increases in the frontogenetic area. Part of the potential energy is converted into kinetic energy.

1. THE GROWTH OF SYNOPTIC METEOROLOGY

One may say that progress in synoptic meteorology depends on the development of three basic factors: firstly, the development of instruments, which give us direct information of atmospheric conditions ; secondly, the organization of observations in a three dimensional atmosphere, *i.e.*, the geographical branch of meteorology ; thirdly, the theoretical study of the atmosphere's physical laws. The complexity of nature causes the third factor to be of less importance than we should wish. Theoreticians like Helmholtz and Margules have solved atmospheric problems without having accurate observations but these are rather uncommon cases. While the theoreticians may explain and give exact expressions for empirical facts and give hints or indicate what the physicist should investigate, our knowledge of atmospheric structure is, I believe, primarily dependent on the development of instruments.

The work by V. Bjerknes and his collaborators in Bergen about 1920 led to an increased number of observations and a careful study of many kinds of weather phenomena. Especially through the introduction of the concept of fronts one succeeded in visualizing processes of a complicated nature. The empirical patterns made in Bergen are applied in all extratropical regions and also in tropical areas. We all know the cyclone model in which the warm air glides up along the warm front, giving cloud systems and precipitation, whereas behind the cold front the subsiding and drying cold air is gaining ground from the occluding warm air. Most of us, I believe, have accepted this model, perhaps with some modifications. The results of the theoretical work in Bergen were less remarkable if one considers their applicability in synoptic meteorology. The cyclone model was based principally on surface observations including observations of clouds and precipitation but of course it was desirable to confirm the results with upper air data. In the thirties Dines's and Jaumotte's meteorographs were used for serial ascents. Vertical time cross-sections, as substitutes for spatial cross-sections, showed the distribution of temperature and humidity. But new inventions awaited the meteorologists. The airplane carried recording instruments and the pilot was able to observe weather phenomena that could not be recorded. From that point of view airplane sounding is a method superior to other sounding methods. The data from the airplane soundings could be made available to the forecaster in a short time, before it lost its prognostic value. Then radio technique was applied, the first radio-sonde appeared in 1928 and the number increased gradually. A synoptic aerology was established which developed slowly at first but during the war very quickly. After the war some of the observations ceased but others were added and on the whole there has been a continuous increase of observations. Equally important is the instrumental development which occurred before and during the war. By means of radar and other electronic devices the wind is now measured with great accuracy in any weather.

From such observations and the careful investigations of selected cases by J. Bjerknes, Palmén, Van Mieghem and others we have acquired a good knowledge of the thermal structure of highs and lows in general and of fronts in more detail. We know that lows and highs go through a process of development ; they are born, grow, stagnate, decline and die, and in their different stages they show varying structure. The individual weather systems may have any intensity and the course of evolution varies from one case to another. While members of the Bergen school have tried to find criteria for the development of cyclones from the slope of the fronts or the intensity of the frontal zone or the wave length of a young disturbance, other theoreticians, such as Rossby, have used new observations over large parts of the hemisphere to show and emphasize the influence of one system on another. I think it is obvious to anyone working with synoptic upper air charts that there is a definite interaction between different systems, a fact that can be used in forecasting. On the other hand, it is to some extent feasible to forecast a new development knowing just one weather system and nothing of the conditions in adjacent systems. But in such a case one ought to know the position and intensity of isallobaric systems rather than the slope or intensity of fronts. Starting from infinitesimally small disturbances there is in my opinion little hope for a successful forecast. Thus the study of weather systems is a branch of the study of the general circulation.

Like the thermal structure, the horizontal distribution of the wind is also quite well known, both from direct measurements and by means of pressure observations at the ground and temperature observations in the upper air. In such ways it has been found that around the whole hemisphere there is a strong fairly narrow westerly current roughly in the 10-km level. This stream, comparable to a meandering river, has been called the jet stream. Like the frontal zone this jet is as we shall see especially strengthened on the southern side of deep troughs which, in the upper troposphere, lie vertically above, or, in deepening systems, slightly to the west of cyclones at the surface. The existence of the jet — or the frontal zone — is in itself remarkable. In addition it has been stated, or at least suggested, that the hydrodynamic instability on the southern side of the jet could lead to the development of cyclones. However, no synoptic proof of this has been given as far as I know.

Humidity observations are still of low quality and they will probably remain so until a new instrument is designed that is reliable, cheap and simple. Prof. Dobson's frost-point hygrometer has already given very interesting results on the subsidence that occurs in cold tropospheric air as well as in the stratosphere in our latitudes, but many more observations from this or similar instruments are required to be made and related to various synoptic situations. Cloud observations from airplanes show how the clouds follow the warm front but appear below as well as above the frontal surface, indicating that there is an ascending motion even below the front in the prefrontal part of the frontal zone. The effect of precipitation on the formation of clouds below the front is, I believe, very limited. At cold fronts the clouds are not connected with the sloping front, which is normally of very little significance, but exist vertically above or ahead of the surface front. This indicates that instability clouds at the cold front are often not to be explained by an ascent of the warm air forced by the cold air, but by instability due to over-running cold air in the upper levels. This leads to strong ascending local currents in an area where the mean motion is directed downwards.

Vertical winds are not generally measurable. The large-scale vertical motion of the atmosphere can be computed under certain assumptions from the temperature and wind field on two consecutive upper air charts (Fleagle 1947, Miller 1948), or from other methods using the horizontal divergence or the intensity of precipitation about which we have read recently in the *Quarterly Journal*, (Bannon and others, 1949), and we know that values of 5-10 cm/sec are common. One of my

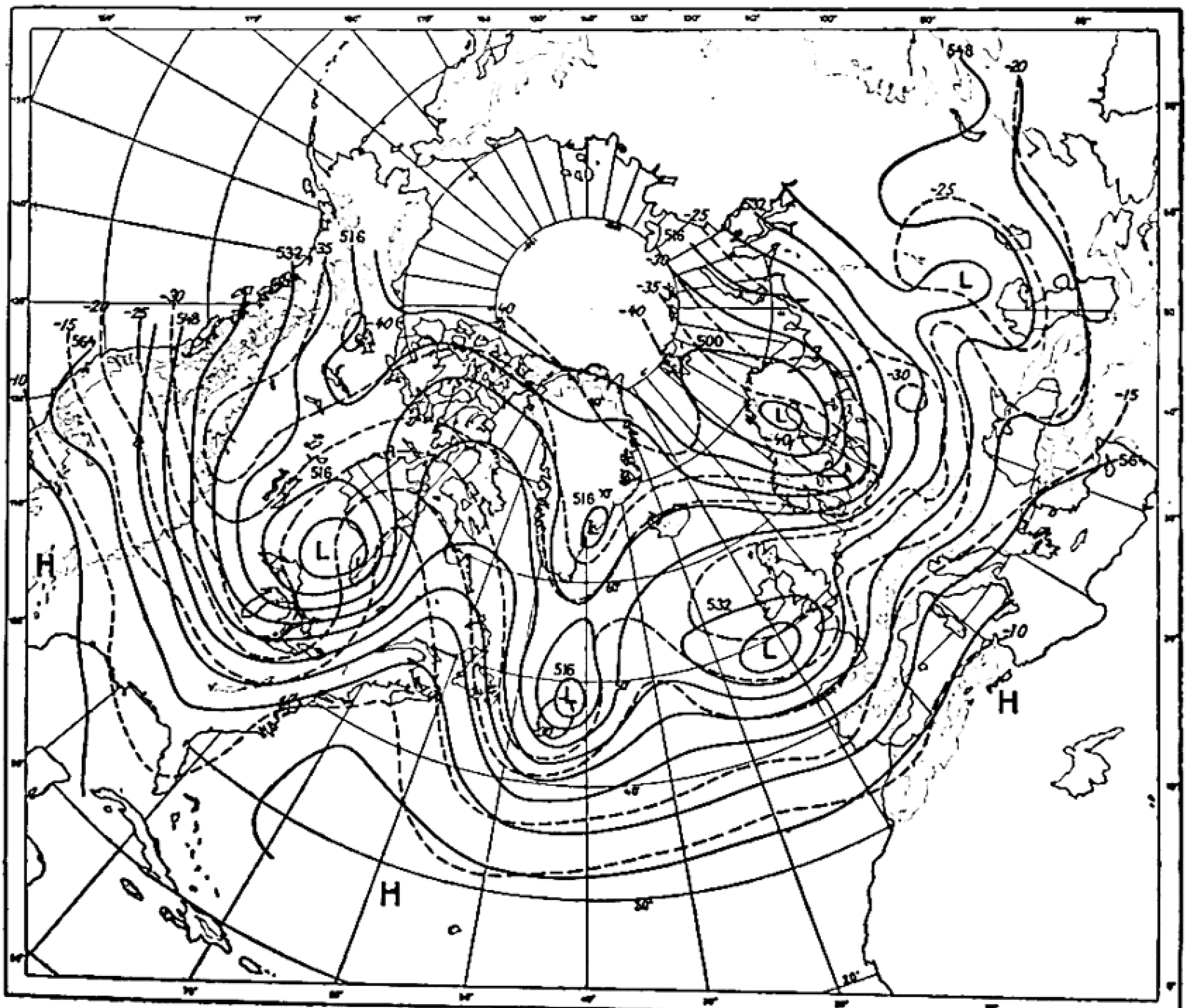

Figure 1. Geopotentials (in tens of geopotential metres) and isotherms in the 500-mb level 7 November 1948, 1500 G.M.T.

Swedish colleagues, Mr. M. Eriksson, has estimated the vertical motion in various weather situations by a statistical study of the vertical rate of ascent of balloons, and his results are in good agreement with those mentioned.

There are a great many problems connected with the structure of weather systems, some of which I have lightly touched upon. Many of these problems will possibly not be solved until new instruments are designed and a large number of observations have been made. However, we may discuss problems without solving them and a few diagrams follow which I believe have some bearing on the problems of the formation of fronts and of jet streams, as well as of vertical motion in connection with these phenomena.

2. Discussion of selected synoptic situations

Fig. 1 shows a situation in which there is a region over the North Atlantic with a rather weak meridional temperature gradient (in a south-north direction). No significant front can be found. One can see that the distribution of pressure and temperature are such that the isotherms will in time become more concentrated near Iceland and to east of it ; according to Bergeron (1928) there is frontogenesis or, to use Namias's terminology, there is confluence. In connection with the

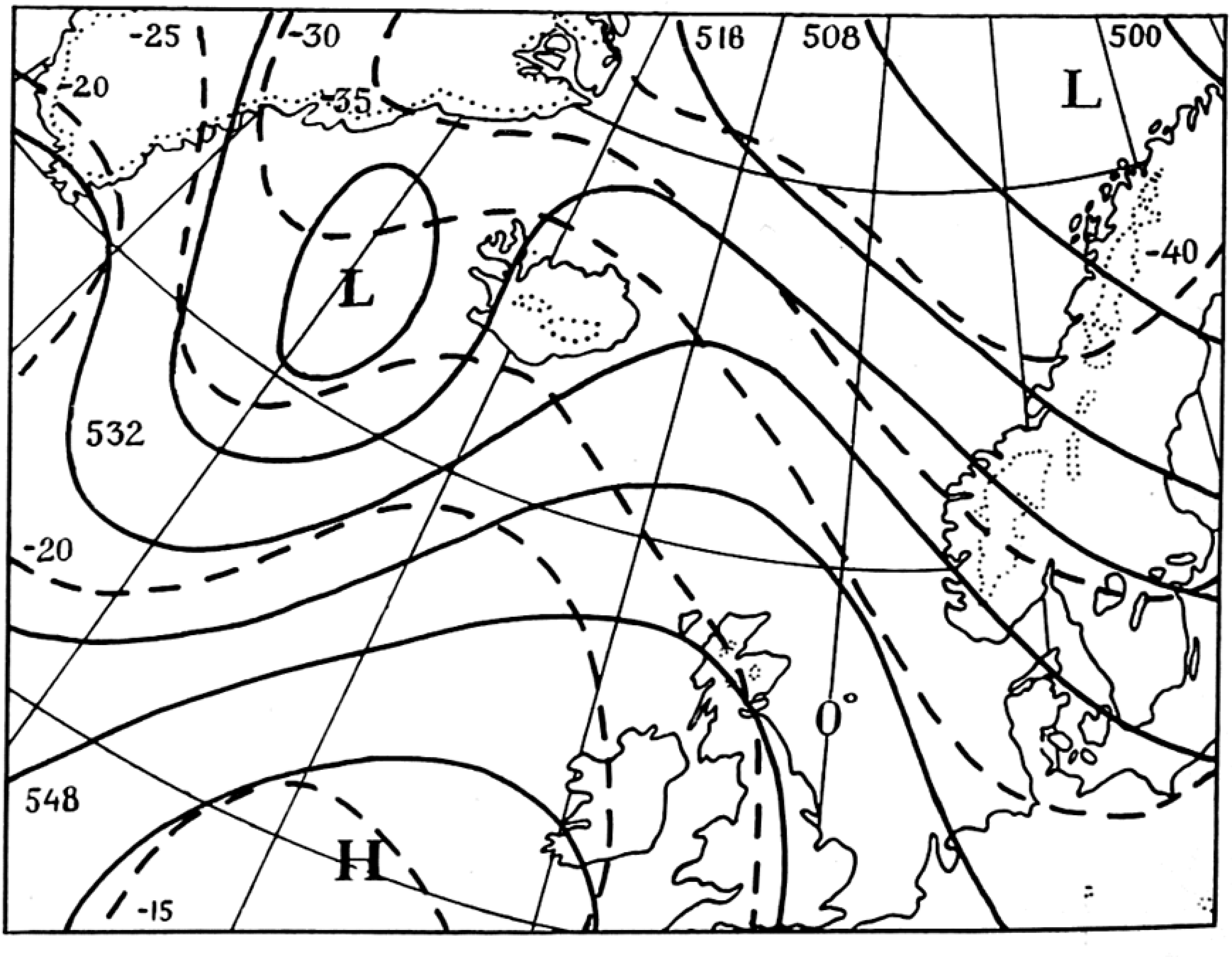

Figure 2. Geopotentials (in tens of geopotential metres) and isotherms in the 500-mb level 8 November 1948, 1500 G.M.T., showing a confluence over the Northern Atlantic.

development over the Middle West, warm air is brought up to the area west of Greenland and later on this leads to an increased northerly current advecting cold air east of Greenland. Such processes are well known (J. Namias and P. F. Clapp 1949) and are very useful in forecasting.

Fig. 2 shows the situation 24 hours later and we can now discern a frontal zone and jet which will become still sharper, since there is continued frontogenesis. But the concentration of the isotherms is less pronounced than would be obtained from horizontal advection and this is so because the air which has passed through the warm-advection area has risen and cooled while the air which went through the cold-advection area has subsided and warmed adiabatically. This is a normal process. It is difficult to give a theoretical explanation but it is a useful rule. In a systematic study of the vertical motion over England in November 1947, my colleague Mr. Oredsson and I have found that this rule, *viz.*, warm-advection ascending motion, cold-advection descending motion, held in all cases studied in the 700-mb and 500-mb levels. We used the fine British observations made at 6-hour intervals only, drew charts for observational times and computed wind trajectories, using observed winds or gradient winds. The vertical lapse rate was known and the difference between real and computed temperature change was assumed to be a result only of vertical motion. Similar computations are shown in Fig. 3. The values give the vertical motion during the last 12 hours of air particles the trajectories of which end on the vertical cross-section. In the northern parts, in the trough north-east of the jet, there seems to be descending motion aloft. We see that there is a strong ascending motion on the east side of the ridge where the warm advection takes place. The ascent is rather small in spite of the strong advection in the 500-mb level, but increases with height and reaches high values in the stratosphere. This cooling of the stratosphere is more clearly demonstrated in Figs. 4 and 5 which give the flow and the isotherms in the 200-mb level. Fig. 5 shows an area enclosed by the isotherm of —70°C while the lowest temperature reported on 7 November was —63°C. The displacement of the cold area towards east is very small although the west wind is about 80 km/hr. Fig. 6 shows the 500-mb chart after the passage

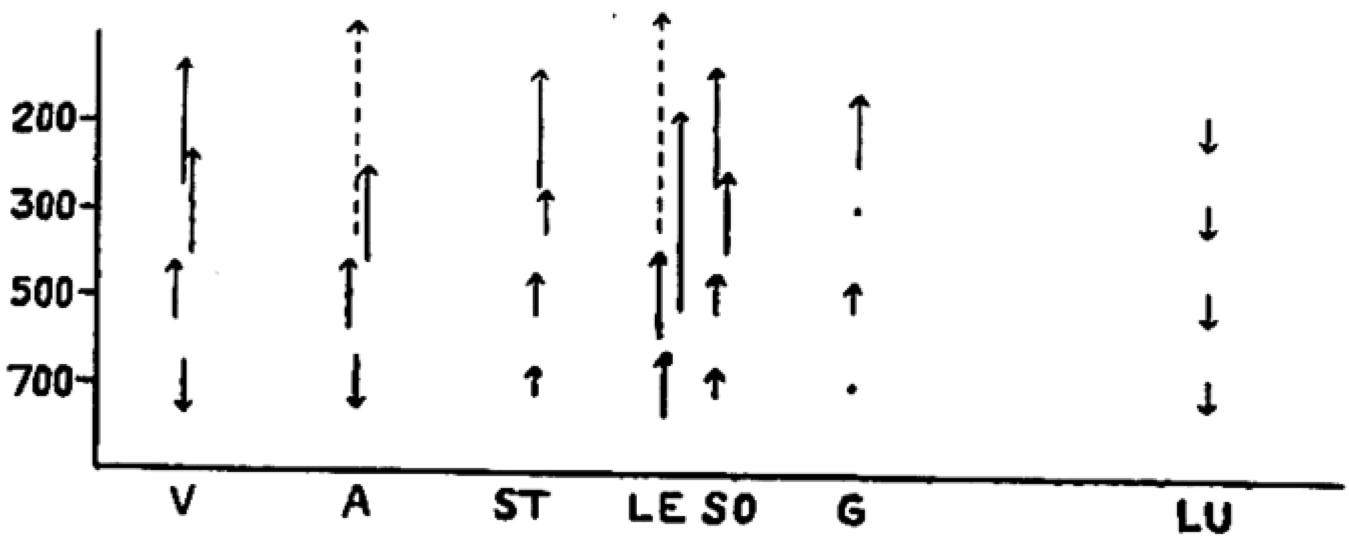

Figure 3. 12-hourly vertical displacement of air particles the trajectories of which end on the cross-section : Valentia, Aldergrove, Stornoway, Leuchars, Lerwick, Sola, Gardermoen, Luleå, on 9 November 1948, 0300 G.M.T.

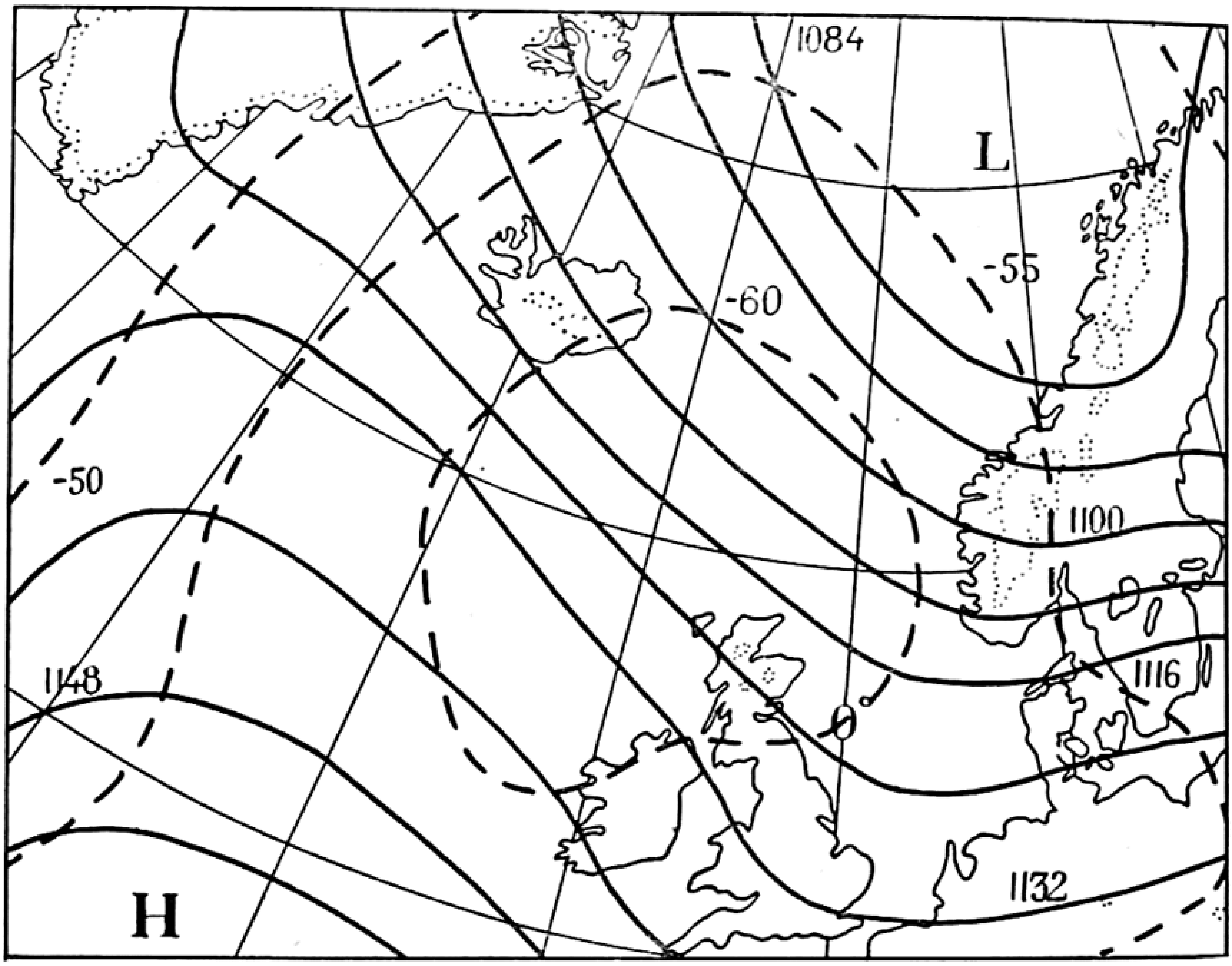

Figure 4. Geopotential (in tens of geopotential metres) and isotherms in the 200-mb level 7 November 1948, 1500 G.M.T.

of a disturbance from Southern Greenland to Central Sweden. We have measured velocities of the wind of 160 km/hr as compared to 50-70 km/hr on the first day in that region and 100 km/hr in South-Western Greenland. Fig. 7 shows the vertical displacement for 10 November in the same way as Fig. 3. These vertical displacements are rather uncertain in the southern parts where the wind velocity was very high and the origin of the trajectories doubtful. The values in that part seem to be too high. At any rate there is no significant subsidence during the preceding six hours during which the air has been in an area with parallel isobars and isotherms but perhaps earlier in an area with cold-air advection.

A few conclusions can be drawn. The situation on 7 November 1948 is such that strong frontogenesis prevails in the region between Iceland, Scotland and Norway. The thermal wind is increasing in that region as seen from the horizontal advection but on account of vertical motion the increase of the thermal wind is reduced. The real wind speed increases aloft locally as also does the speed of individual particles. There is no marked disturbance at the surface. The circulation about a horizontal axis is in a direct sense, *i.e.*, the colder air sinks and the warmer air rises. A frontal zone, even a front, is formed which would have been still sharper but for the vertical motion. This is contrary to a statement by

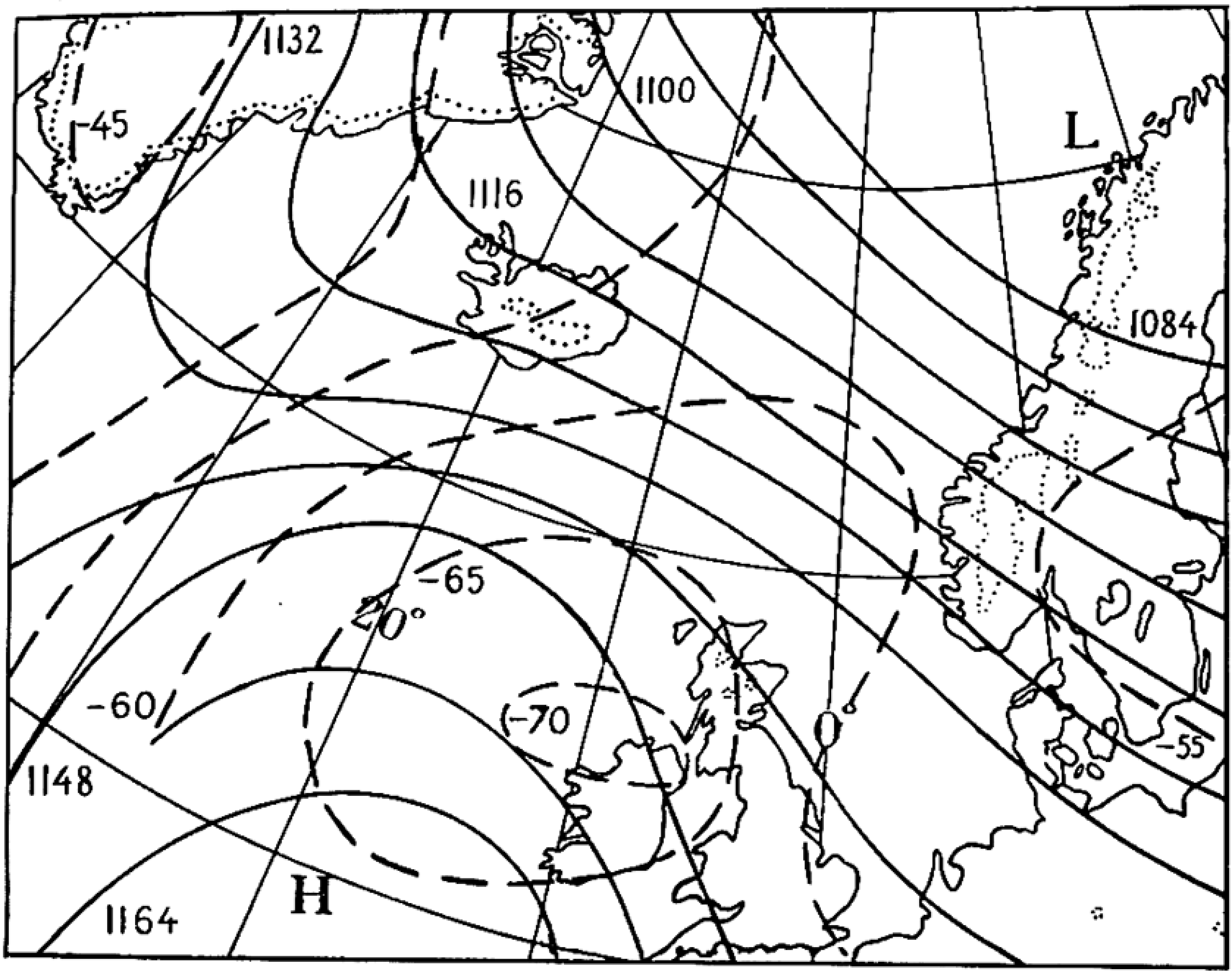

Figure 5. Geopotential (in tens of geopotential metres) and isotherms in the 200-mb level 8 November 1948, 1500 G.M.T.

Palmén and Nagler (1949) that the front in the western parts of a trough is sharpened through indirect circulation. The result of my studies is that the atmospheric machine converts potential energy into kinetic energy with great regularity. Any indirect circulation that may exist in the troposphere is negligible and there is at present no reliable way of confirming its existence. The low situated south-east of Greenland on 7 November was predicted by many weather services to move in the growing jet over the North Atlantic and deepen between Iceland and Norway and at the same time the front was expected to form. This forecast was correct as was also the prediction that after the formation of the jet and the sharp front the weather situation in that area should undergo only small changes. The fronts were formed simultaneously with the jet and both fronts and jet were most pronounced at the end of the development and thus they can hardly be the cause of the development.

One should study carefully the regions in which confluence prevails as in these regions frontal zones, *i.e.*, solenoids are formed and vertical motions take place which directly influence the weather. In these regions also potential energy is converted into kinetic energy and the jet stream increases, influencing the general

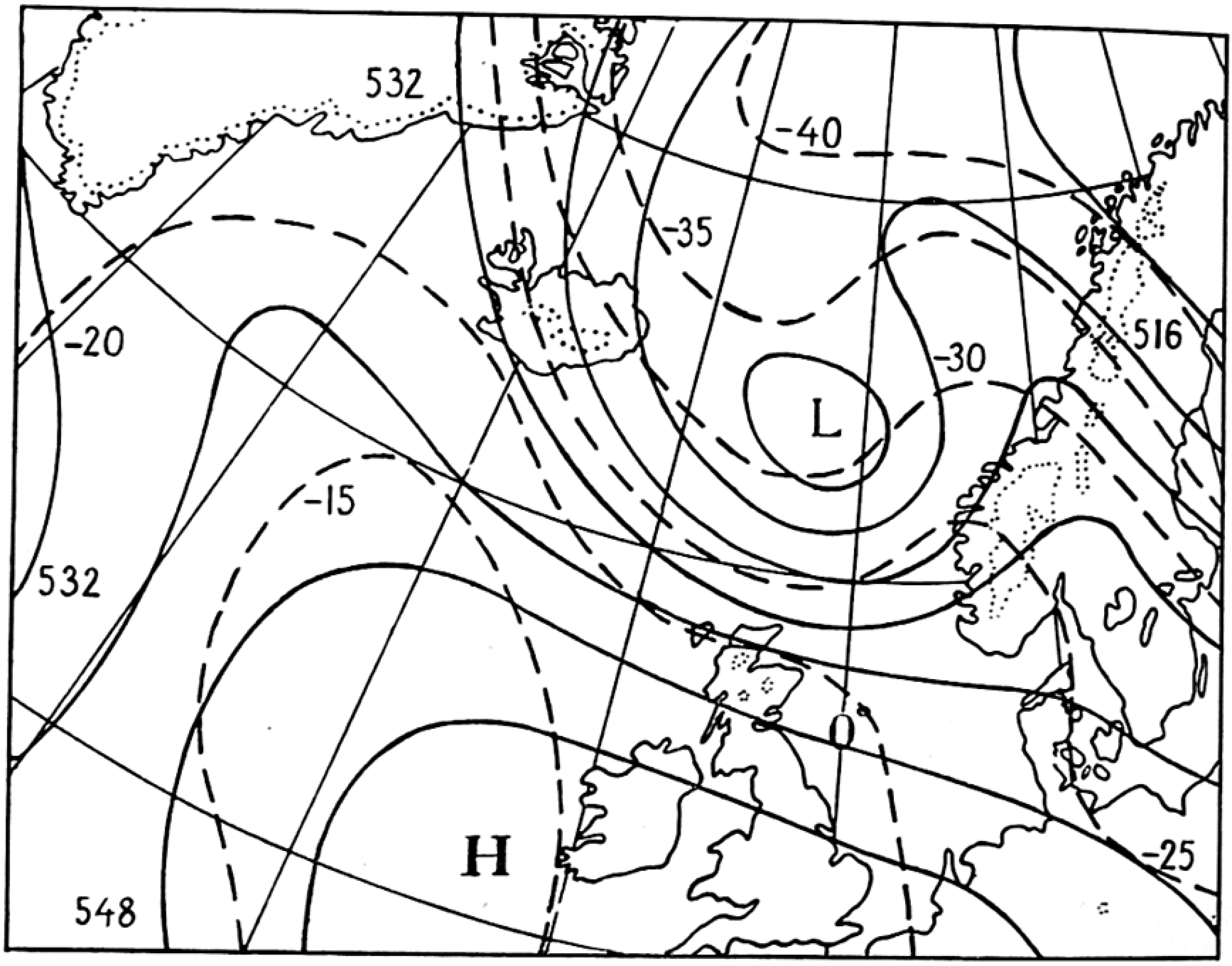

Figure 6. Geopotentials (in tens of geopotential metres) and isotherms in the 500-mb level 9 November 1948, 1500 G.M.T.

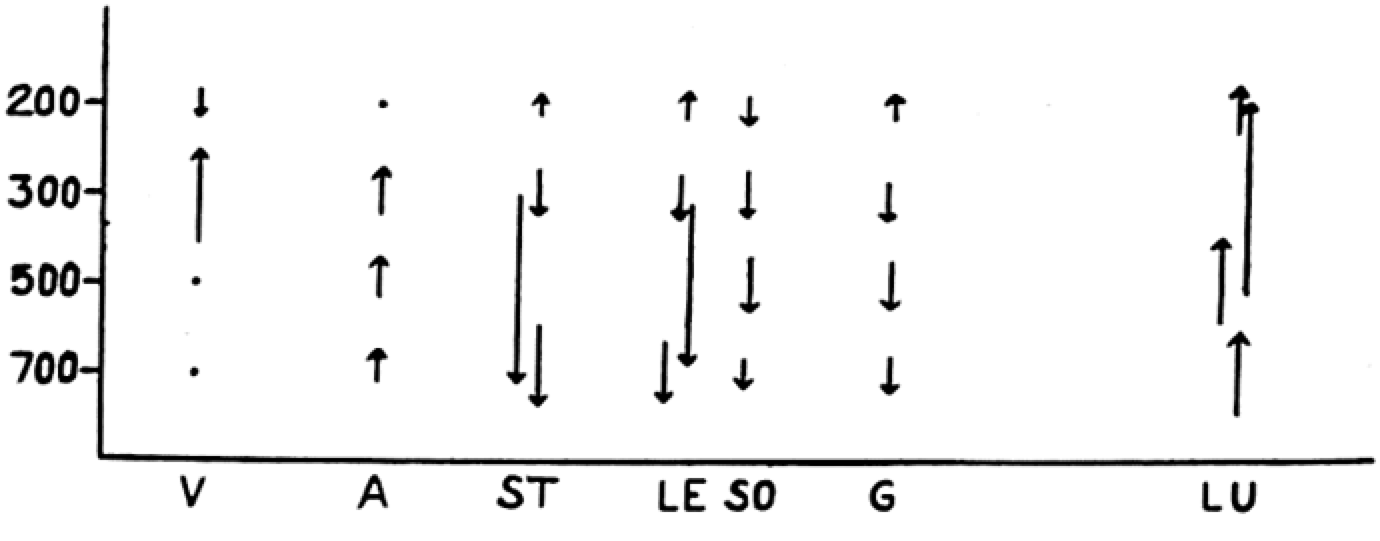

Figure 7. 12-hourly vertical displacement of air particles the trajectories of which end on the cross-section : Valentia, Aldergrove, Stornoway, Leuchars, Sola, Gardermoen, Luleå, on 10 November 1948, 0300 G.M.T.

circulation and thus adjacent weather systems. The problem of the formation of confluence areas should also be studied and especially the relation between confluence and transport of momentum and vorticity.

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The structure of North American weather systems*

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SUMMARY

The air masses that affect North America in summer and winter are described first, special attention being paid to the processes of transformation of cold air masses to warmer ones, and *vice versa*. The major frontal systems are next outlined, with particular emphasis on the three-air-mass, or double-wave, theory of major depressions, and the essentially three-dimensional continuity of major frontal surfaces.

The chief features of frontal topography over North America are presented — the upper cold front, its structure and frontolysis ; the upper warm front, its structure and significance for frontogenesis ; the cold dome, its structure, formation and dissolution ; and the warm pocket, its structure, formation and dissolution. These aspects of frontal topography are illustrated by reference to the synoptic situation over North America on 5 January 1949.

In conclusion, the feasibility and merits of a complete three-dimensional frontal and air mass analysis are discussed. The implications of such an analysis for the construction of prognostic and upper level charts are indicated.

1. NORTH AMERICAN AIR MASSES

The classical studies of North American air mass properties (Willett 1934, and Showalter 1939) were made at a time when upper air data were not nearly so extensive in the vertical or horizontal as they have been in recent years. In particular, the development of a network of radiosonde stations over Alaska, Northern Canada, and Greenland has resulted in an increased knowledge concerning Arctic air masses and the Polar air masses into which they are almost inevitably transmuted.

Following Bergeron (1928) and Schinze (1932), there appear to be two distinct types of Arctic air in winter — continental Arctic air (cA) which is characterized by very low temperatures, especially near the ground, and very small lapse rates ; and maritime Arctic air (mA) characterized by very low temperatures at high levels (*i.e.* 400-500 mb) and very steep lapse rates, producing temperatures at 1,000 mb of the order of 0°C. Representative temperatures at various levels for the different

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North American air masses will be presented in tabular form at the close of this section. Maritime Arctic air appears to be produced from continental Arctic air in three different ways : first, by a short trajectory over a cold water surface ; second, by a longer trajectory over a large body of water covered by ice, through which, according to Malmgren (1927), a considerable heat flux may be conducted ; and finally, by a long trajectory over a land surface characterized by an excess of non-reflected insolation over outgoing radiation (*i.e.*, at low latitudes or over a snow-free surface). The continental Arctic air itself may be presumed to represent a state of radiative quasi-equilibrium in the presence of a snow surface and the absence of appreciable insolation.

Analysis of winter synoptic charts reveals the presence of several species of Polar air. Continental Polar air (cP), cool at all levels with small lapse rates, appears to arise from the slow modification at all levels of continental Arctic air moving southward over a snow surface. If such air leaves a continental area and passes over a relatively warm water surface the air is rapidly modified and attains maritime Polar (mP) characteristics — *i.e.*, fairly steep lapse rates with relatively low temperatures aloft and relatively high temperatures near the surface. However, any extended sojourn of the continental air at low latitudes in the absence of a snow cover will produce a similar thermal modification, although the air will be somewhat drier. Since the properties and existence of fronts depend essentially on virtual temperature discontinuities (in practice of the first order as indicated by Bjerknes (1932) and Palmén (1946)), it follows that a purely thermal labelling of air masses will be the most effective in maintaining continuity in synoptic analysis. In other words, an air mass contiguous to a given front should be given the same designation at all points along the front, regardless of the possible differences in trajectory which might exist. Such a thermal definition of an air mass is permissible at the present time since indirect aerology is no longer a significant factor in weather forecasting. Moreover, air masses which would invariably be termed maritime from their trajectories frequently appear relatively dry at levels above 700-800 mb.

The chief differences between maritime Arctic and maritime Polar air masses are of degree only so that in theory a continuous graduation of properties could be expected. In practice, however, maritime air masses are quasi-homogeneous in character. At times, in addition to a distinct maritime Arctic air mass, there will exist two maritime Polar air masses manifesting a significant temperature differential between them. They may be designated as warm maritime Polar and cold maritime Polar ; they may be abbreviated to wmP and cmP, respectively, disregarding etymological inconsistencies.

Tropical air of the maritime type can be produced if maritime Polar air has a trajectory over tropical waters. Such a trajectory is usually accompanied by a net subsidence at high levels so that the resulting mean lapse rate generally is more stable than the pseudo-adiabatic. If strong subsidence occurs at all levels above the ground in a relatively warm maritime Polar air mass and if turbulent or convective processes are in operation in the lower levels the air mass will attain essentially tropical characteristics, even though it will remain considerably drier than true maritime Tropical air.

It is rather obvious that air-mass modification will not proceed entirely towards the formation of warmer air masses ; hence it is of significance to consider the reverse transformations. Cooling from below is a very slow process since it results in stabilization, minimizing eddy diffusion of heat. Simultaneously, however, radiative cooling at upper levels and radiative diffusion of heat are accelerated. Thus at high latitudes over an ocean area a tropical air mass will in time attain maritime Polar properties and at high latitudes over a land area a maritime Polar or maritime Arctic air mass will, in winter, eventually exhibit continental Arctic characteristics.

A further process by which an air mass may be transformed into a colder species is ascending motion. This process is in part responsible for the presence of cooler air above a frontal surface than in the warm air mass proper and is sometimes known as " pre-frontal cooling." It has been described by many authors, including Bjerknes and Palmén (1937) and Van Mieghem (1939). The magnitude of this cooling effect increases with the stability of the air and with the magnitude of the ascending currents. Marked cooling is noted most frequently in North America over the western United States, where it is doubtless reinforced by orographic influences. To the west of the Continental Divide 12-hour cooling of the order of 5-8°C prior to a cold-frontal passage at the ground is often observed. This cooling is a maximum near 500-600 mb and is negligible above 300 mb or below 800 mb. The cooled air will of course be transported by advection, even if the ascending currents at the front disappear at some time later. It may be concluded that the interconversion of air masses can arise from vertical motions and by the operation of solar and terrestrial radiation. These transitions represent the chief processes leading to and balancing the net heat gain at low latitudes and the net heat loss at high latitudes, the air masses involved representing the eddies of Defant (1921).

As a result of the greatly increased insolation in polar regions in summer as compared to winter and the very slight increase in subtropical regions the total temperature differential between the warmest and the coldest air masses is far greater in winter than in summer. It is only natural, then, that the number of air masses in summer should be less than that in winter. The coldest air in summer will be found over the Arctic oceans and will be directly comparable to winter maritime Arctic air, although somewhat warmer, especially at high levels. In summer there exists no direct equivalent for winter continental Arctic or continental Polar air masses ; instead, all air masses will have lapse rates characteristic of maritime air. Over the continent the Polar air will be essentially maritime Polar, the large amounts of evaporation and transpiration being sufficient to render the air relatively moist.

In summer the South-Western United States, by reason of excessive aridity, insolation, and subsidence at intermediate levels, provides a source region for continental Tropical air (cT). This air is warmer than maritime Tropical air (mT) in the lower layers and has a typical lapse rate slightly less than the dry adiabatic, so that at high levels the temperature difference disappears. In summer there is frequently observed a marked discontinuity in dew point west of the Gulf of Mexico but any surface of transition which may exist does not appear to have any dynamic significance.

In Table I there are presented typical summer and winter temperatures (in °C) for the various air masses at selected pressure levels, applicable to the middle latitudes, say 45-50°N. In any particular case considerable variation from these values would be possible, so that these data can only be used as a very rough guide.

TABLE I. Typical temperatures (°C) in winter and summer air masses

Air Mass	cA	mA	cP	cmP	wmP	mT	mA	mP	mT	cT
Level (mb)			Winter					Summer		
400	—49	—49	—41	—42	—35	—28	—42	—31	—18	—22
500	—42	—38	—33	—30	—23	—17	—33	—19	— 8	—11
700	—33	—21	—20	—12	— 4	0	—14	0	+ 8	+10
850	—31	—10	—18	— 1	+ 5	+10	— 4	+11	+18	+22

2. North American frontal systems

As indicated by Petterssen (1940) the major fronts over North America and adjacent oceans are the Pacific Polar front, the Atlantic Polar front, and the Arctic front. The air masses involved in these frontal systems depend in part on geographic location and in part on the circulation index (Allen 1940) prevailing in the region of the front. With a low index (strongly meridional flow) both very cold and very warm air will be drawn into the orbit of a frontal wave. The warm-sector air of the Polar front will then generally be tropical ; immediately to the rear of the front maritime Polar air is encountered, possibly with a second front, roughly parallel to the Polar front, separating warm maritime Polar air from cold maritime Polar air. By analogy with tropical practice this front might be termed the " inter-Polar " front. Under high index conditions (quasi-zonal flow) the Polar front appears to separate a mixture of Tropical air and warm maritime Polar air from cold maritime Polar air.

In general the Arctic front separates cold maritime Polar air from continental Polar air over land areas (usually a mixture of continental and maritime Arctic air masses) and cold maritime Polar air from maritime Arctic air over northern water areas. The inter-Arctic front of Sverdrup (1933) may at times be located as a definite entity and separates continental Arctic air from either maritime Arctic air or continental Polar air (*i.e.*, continental Arctic air modified by a land trajectory).

In winter, for low index conditions, major depressions usually form simultaneously on both the Polar and Arctic fronts, which are generally fairly close together. With high index conditions the two frontal systems may be separated at the surface by as much as 1000 mi, with the major lows forming on either front. Nevertheless, significant waves on the Arctic front rarely if ever develop unless there exists at higher levels above the surface front a distinct warm sector projecting northward from a wave on the Polar front. Such a development appears to be related to the fact that significant migratory upper-level troughs are associated with actively developing waves (*i.e.*, of large amplitude) on the Polar front. Thus, regardless of the index conditions, it appears that the " in-phase " development of waves on both the Polar and Arctic fronts is necessary for the formation of a deep cyclone. This hypothesis has much in common with the *dreimasseneck* theory of Rodewald (1936) for tropical cyclones, although the latter is not as well substantiated by observational data as the present theory for extra-tropical cyclones. It may be noted that there is some evidence to support the existence of a double maximum of zonal wind component in a north-south wind cross-section across a frontal depression. Such a " split jet stream " is in accord with the double frontal behaviour of major frontal cyclones.

In the summer frontal waves may form and develop on either the Polar front (separating Tropical air from maritime Polar air) or the Arctic front (separating maritime Polar air from maritime Arctic air). These two frontal systems are usually well separated, rather similar to a winter high index condition, although displaced to the north and with less cyclonic activity. The major depressions in the summer months also obey the three-air-mass or double-wave theory of cyclonic development. This theory may be considered as a logical consequence of the computations of Margules (1903) on the " energy of storms," in which he found the kinetic energy developed to be proportional to the temperature difference existing between the air masses involved. If three or more air masses participate in a joint cyclonic development it follows that in general a more intense depression will be formed than if only two air masses are involved.

At all times of the year the Polar and Arctic fronts exhibit what is essentially a three-dimensional continuity. Their location on the surface map is frequently rather difficult in many sectors but this difficulty is not experienced to the same degree at upper levels. In winter the frontal contours separating warm maritime polar air from cold maritime polar air can generally be shown as continuous over the entire area analyzed, up to the tropopause. A well-defined frontal surface separating Tropical air from warm maritime Polar air is seldom experienced over the eastern North Pacific Ocean or the western half of the North American continent. Over the western North Atlantic Ocean and over the eastern sector of the continent this front is often well-marked, but generally only in the lower half of the troposphere. Thus in winter the major set of frontal contours delimits the warm maritime Polar air ; in summer, on the other hand, the major set delimits true Tropical air.

At all times of the year a significant set of frontal contours separates maritime Polar air from Arctic air. In oceanic areas three-dimensional continuity may not be preserved, as a result of rapid modifications. Such or similar air mass transformations may affect any frontal surface and it is important to realize when and where they are occurring since they must automatically lead to the three-dimensional frontolysis of a sector of an existing front and its simultaneous three-dimensional frontogenesis in another region. The inter-Arctic front is sharpest in the lower layers and ceases to be of significance above 600 mb ; its horizontal extent is also in general rather restricted. Fronts of limited three-dimensional continuity such as the latter will still, of course, be significant for local weather and temperature régimes but are probably not potent factors in large-scale developments.

3. FRONTAL TOPOGRAPHY

(a) *Upper cold fronts*

Waves on the significant frontal surfaces have amplitudes which often change little with height so that well-developed waves preserve their sinusoidal nature to 400 or 300 mb. In the case of occluded depressions there will be present an elongated warm sector at upper levels which may extend beyond the surface low-pressure centre. This will appear, in three dimensions, as a relatively narrow trough of warm air ; on an east-west cross-section to the north of the surface warm sector this trough will generally have a V-type configuration. On the surface chart the axis of the warm air V-trough will be evident as a pressure trough, marked by those discontinuities in pressure tendency and weather which would be anticipated from the replacement of warm air by cold air at the upper levels. This axis line may advantageously be represented on the surface map as an upper cold front as suggested by Lichtblau (1936), if it represents a significant line of weather discontinuity and if its position would not readily be predictable from the surface frontal pattern alone. Illustrative examples of the structure of both upper cold and upper warm fronts will be presented in sub-section (c).

If air mass differences exist to the north of the occluded surface warm sector appropriate surface cold or warm fronts (or both) will naturally be represented on the base chart, in addition to the upper cold front representing the upper trough of warm air. If only a single air mass exists in the vicinity of the upper cold front differences in trajectories may produce an occluded frontal surface in the lower layers which will, however, be considerably less important from a dynamic or a synoptic point of view than the upper cold front.

Significant upper cold fronts occur in all regions in, and in the vicinity of, North America. On the west coast of the continent, in particular, they play a major role in frontal configurations. They may extend as far as 1500 mi from the surface warm sector in a marked pressure trough which generally contains no surface fronts. The passage of an upper cold front results in strong cooling at upper levels with frequently little or no temperature changes in the lower levels. The recognition of the true structure of these upper cold fronts is important for several reasons : first, the associated weather is caused chiefly by the warm air aloft, whose presence would often not be anticipated from the surface frontal pattern alone ; second, the associated pressure trough may extend beyond the intersection of the warm air trough with the tropopause, causing confusion in analysis unless actual frontal contours are constructed or deduced qualitatively ; and third, the release of an active wave on the Arctic front requires the presence of an upper cold front, *i.e.*, the definite presence aloft of a warm sector intrusion from the Polar front.

(b) *Upper warm fronts*

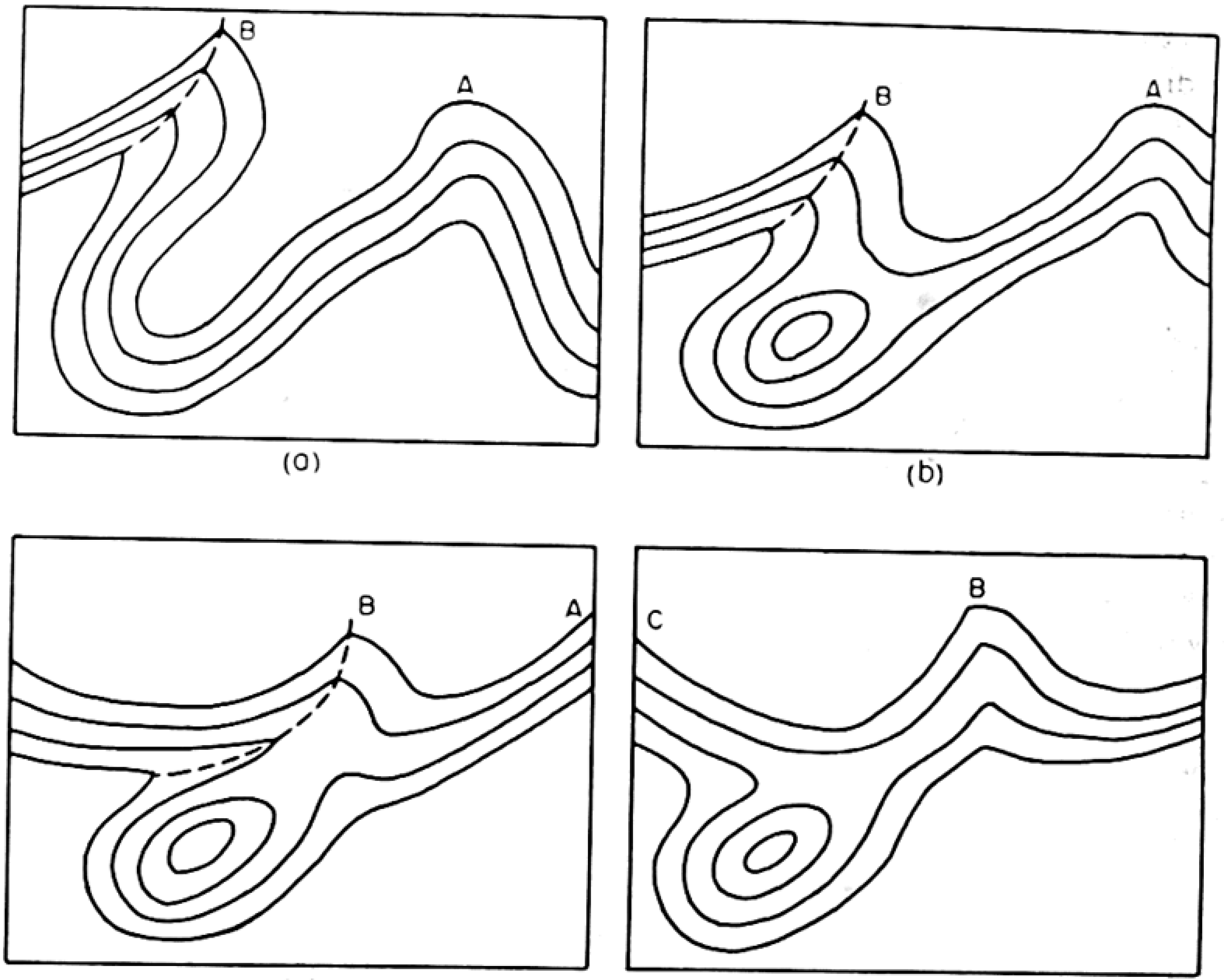
Warm or quasi-stationary fronts frequently do not show a uniform slope with height. In the lower layers they may be very shallow and a normal slope may be instituted approximately along a line on the frontal surface. The projected position of this line on the surface chart will then be marked by a pressure trough, which, if moving, will manifest a discontinuity in pressure tendency. If ascending currents are present, considerably more clouds and weather in general will be experienced over the normal slope than over the shallow-slope sector of the frontal surface. If these discontinuities are present the trough line may be represented on the surface chart as an upper warm (or quasi-stationary) front, a designation proposed by Lichtblau (1936) and Weightman (1939). In such cases the original surface front often dissolves and the front redevelops in approximately the position of the upper warm front as a result of turbulent mixing plus radiative heating and convection.

Such phenomena can also take place on a considerably larger scale, being associated with transformations of large sectors of an air mass resulting, in part at least, from strong lateral divergence and subsidence of the cold air in addition to the processes of convection and turbulent transfer in the surface layers. Thus at upper levels frontolysis and frontogenesis may occur, leading to the temporary rupture of otherwise-continuous frontal surfaces. As is the case for surface fronts, frontolysis occurs because of major air mass modifications and not as a result of any kinematic field of motion. Differential temperature advection may change the width or thickness of a frontal transition zone but not the temperature differential between adjacent air masses. Since only vertical motions and non-adiabatic effects can cause air mass transformations, only these processes can be responsible for major frontolytic tendencies. As the given frontal surface re-establishes itself its basic three-dimensional continuity is restored, with no net increase or decrease in the number of significant fronts.

Over North America and the adjacent oceans the upper warm front structure is quite often observed and is especially significant with regard to temperature and weather forecasting, since in such cases the simultaneous frontolysis and frontogenesis described previously cause surface warm fronts to appear to accelerate rapidly during the afternoon hours and later to decelerate rapidly. The trough of the upper warm front is often mistaken for a surface warm front, which leads to a double front structure when in fact such does not exist. The orographically-induced type of upper warm front, which descends to the surface as described by Petterssen (1940), is of frequent occurrence in the mid-western areas of North America. With a surface front roughly parallel to and west of the west coast an upper warm front may develop in the lee trough to the east of the Continental Divide. Cold air often remains in the mountain valleys and over the enclosed plateau regions even after the warm air has descended to the surface to the lee of the mountain chain. This residual air is effectively trapped over the lower terrain and in winter is removed only by the advance of a colder air mass. In summer, however, the residual cold air is rapidly modified by insolational heating.

(c) *Cold domes*

Upper air analyses frequently show the presence of a cold dome of air, isolated at upper levels from the main body of cold air. This dome may become completely isolated at all levels leading to a closed quasi-circular front on the surface map enclosing a cold air mass, a phenomenon which is occasionally observed on the west coast of North America at middle latitudes. The formation, structure, and dissolution of cold domes have been described by many authors, notably Nyberg (1945), Crocker, Godson and Penner (1947), Crocker (1949), Palmén (1949), Phillips (1949), Palmén and Nagler (1949), and Hsieh (1949).



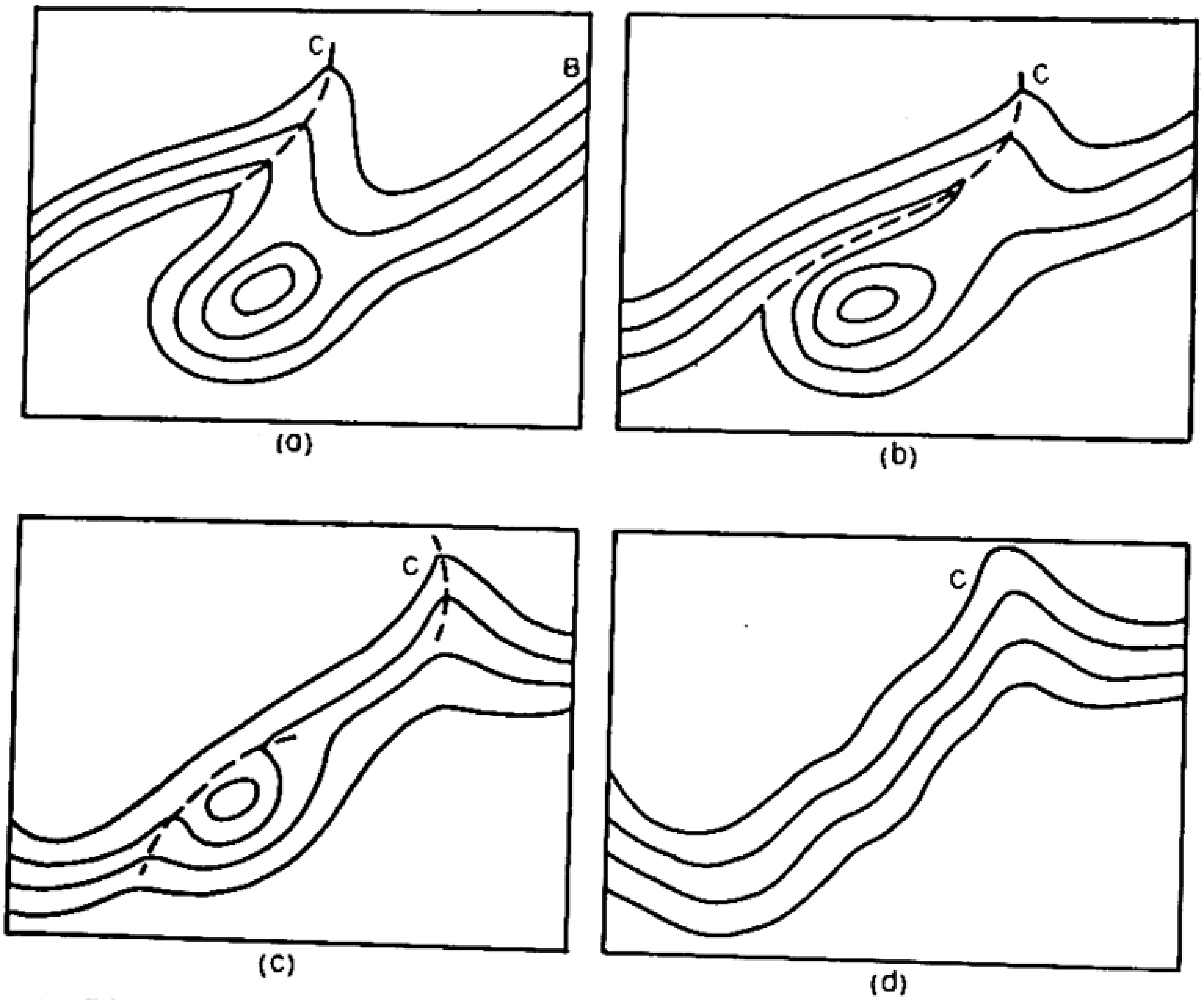
(a) (b)

(c) (d)

Figure 1. Formation and maintenance of a cold dome (frontal contours for 1,000, 800, 600 and 400 mb).

The formation (or isolation) of a cold dome is illustrated by the sequence of schematic frontal contour charts of Fig. 1. From south to north the contours represent the intersection of the frontal surface with the pressure surfaces of 1,000, 800, 600 and 400 mb. Fig. 1 (a) represents the typical frontal configuration immediately prior to the " cutting-off " process. To the rear of wave A cold air has pushed far southward, typical of a low index circulation. The following wave B advances rapidly eastward and, by a combination of subsidence and advection, a belt of warm air is established to the north of the southern sector of the cold air mass (Fig. 1 (b)). The dashed line of wave B marks the position of the upper cold front of that wave and, as can be seen, it represents the locus of the wave crests at different pressure levels. As wave B advances further eastward the tongue of warm air is effectively pinched off by the cold dome in the lower layers ; at higher levels the wave moves unimpeded eastward (Fig. 1 (c)). Eventually the upper cold front disappears as a significant entity in the lower layers (Fig. 1 (d)), while wave B has been transferred from the west to the east side of the dome. When this occurs, the pre-existent wave on the east side of the dome, hitherto poorly developed, now develops rapidly since a deep and extensive body of cold air is available for the transformation of potential to kinetic energy.

Fig. 2 shows a sequence of schematic frontal contour charts illustrative of one process attending the dissolution of a cold dome, namely reamalgamation with the parent body of cold air. Wave C of Fig. 2 (a) may be considered to be either the



(a) (b)

(c) (d)

Figure 2. Dissolution of a cold dome by amalgamation (frontal contours for 1,000, 800, 600 and 400 mb).

wave following wave B of Fig. 1, or else several waves later, previous ones having passed eastward at high levels while dying away at low levels. This wave, as well as its accompanying upper cold front, again indicated by the dashed line, is moving south-eastward at all levels ; previous waves had moved generally eastward. When this occurs the upper cold front effectively climbs up the cold dome destroying both the upper cold front and the cold dome, leaving a normal wave (C of Fig. 2 (d)) with a pronounced southward protrusion of cold air. It may be visualized that at this stage, or at the time of Fig. 2 (c), a rapid development of the surface wave will take place. Without a clear picture of the structure aloft, such a development might appear quite incomprehensible.

Fig. 3 shows the frontal pattern development which might accompany the dissolution of a cold dome by a gradual thermal modification. The entire process would seldom be accomplished during the passage of a single wave eastward unless the cold dome itself were moving eastward. In general cold domes are very slowly moving and may drift in any direction, frequently even westward or southward. If strong subsidence takes place, the original cold air will eventually acquire temperatures very little different from that of the surrounding warm air ; in addition, the dome itself will subside. The requisite horizontal divergence may involve the lateral spreading out of the cold air or may be produced chiefly by the eviction of cold air from the dome northward into the main body of cold air, in the lower

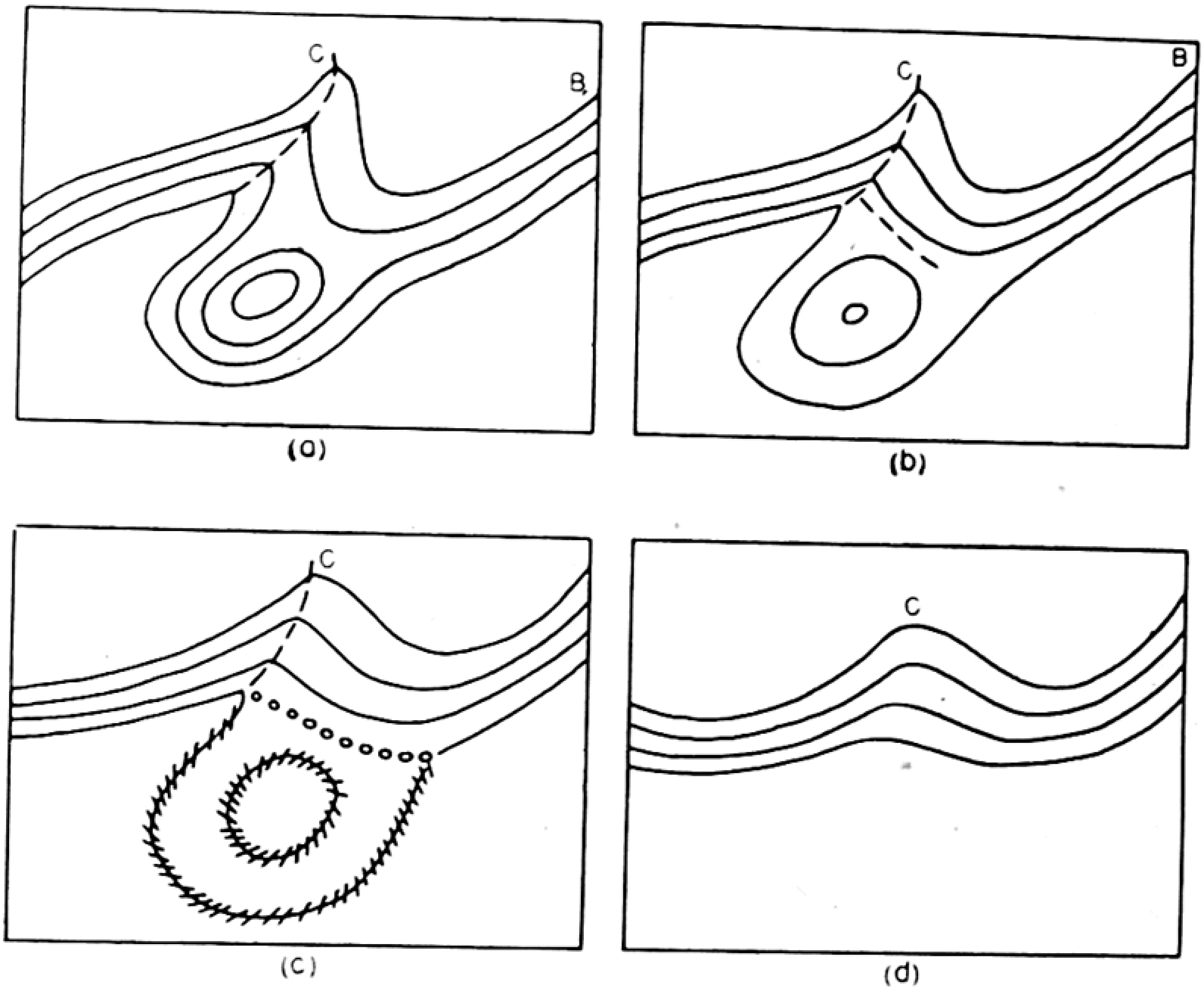


Figure 3. Dissolution of a cold dome by subsidence and air mass transformation (frontal contours for 1,000, 800, 600 and 400 mb).

layers. As the valley of warm air separating the dome from the cold air proper descends, the line of institution of a normal slope may become sufficiently well marked to be represented as an upper warm front, as illustrated in Fig. 3 (b). As the subsided air mixes with and becomes part of the warm air mass the closed fronts encircling the isolated dome will dissolve, the surface front will also undergo frontolysis, and simultaneous surface frontogenesis will take place underneath the valley of warm air, as represented in Fig. 3 (c). Finally only a normal wave pattern remains, as in Fig. 3 (d).

(d) *Warm pockets*

Analogous to the cutting-off process producing a semi-isolated cold dome, part of a warm sector may be trapped by the eastward motion of a cold air mass leading to the formation of a warm pocket, isolated at the lower levels from the main body of warm air. This may be described more explicitly by reference to Fig. 1 (a). If the cold air immediately to the east of the upper cold front moves off rapidly to the east or north-east while the cold air in the southern protrusion moves at a much slower rate the advance eastward of the cold air to the rear of wave B will result in the seclusion of a sector of warm air to the north. Occasionally this pocket becomes completely isolated and distinct even at the surface giving a closed quasi-circular surface front enclosing warm air. This has been observed over the North-West Territories on the Polar front and over the eastern Canadian Archipelago on the Arctic front. The subsequent history of warm pockets is directly comparable to that of cold domes, involving either the eventual reamalgamation with the main warm air mass or else a gradual thermal transformation of the air by processes of radiational cooling and dynamic cooling due to ascent until the original warm air has come into equilibrium with the surrounding colder air mass. In the latter case the warm pocket may itself be lifted bodily, the compensating outflow into the parent body of warm air taking place at the upper levels where isolation has not occurred.

(e) *Illustrative example* — 5 *January* 1949

As an illustrative example of many of the processes and of the elements of frontal structure described here the synoptic situation of 5 January 1949 may be cited. The surface map for this date, at 1530 G.C.T., is shown in Fig. 4. The situation at this time was rather complex, as it would have to be to illustrate a large number of the features described in this section, and must not be considered as entirely representative of winter conditions. Isobars are constructed for each 8 mb and fronts are represented in the conventional manner.

The frontal patterns and the air mass distribution may now be described briefly. A wave on the Polar front is just approaching the west coast and has associated with it a long upper cold front, manifesting a sharp tendency discontinuity. The warm sector air is warm maritime Polar, blending into Tropical air aloft. The air to the west and east of the upper cold front is cold maritime Polar. Another wave on the Polar front is found over the eastern United States, the crest of the wave cutting across the extreme southern section of Ontario. The warm-sector air in this case is maritime Tropical, at least in the lower layers. To the immediate west and east of this warm sector maritime Polar air is experienced. At high levels the only significant front in this area separates warm maritime Polar air from cold maritime Polar air ; the surface position of this front would be almost coincident with that of the Polar front. The front itself is somewhat shallower than the frontal surface delimiting the true Tropical air, the latter becoming very diffuse above about 600 mb.

There are two main waves on the Arctic front, both in phase with similar entities on the Polar front. The wave over the northern Yukon is very well developed, even though it would normally be classed as a stable or " open " wave. The air surrounding the warm sector of cold maritime Polar air is Arctic in character ; only to the east of the wave can the inter-Arctic front be identified, on which an occluded low had recently developed. It is interesting to note that the air over the central United States and the western United States is Arctic in the lower layers. Over western Canada the surface air is cold maritime polar and a narrow tongue of this air has been transported southward in the circulation about the Great Basin anticyclone. The Arctic frontal wave crested just north of western Lake Superior also has Arctic air as its cold air mass. A well-defined cold front in the north-east separates continental Arctic air from maritime Arctic air. This frontal surface can be located at upper levels over the entire northern sector of the map but, except for the sector west of Hudson Bay, its position at the surface cannot uniquely be

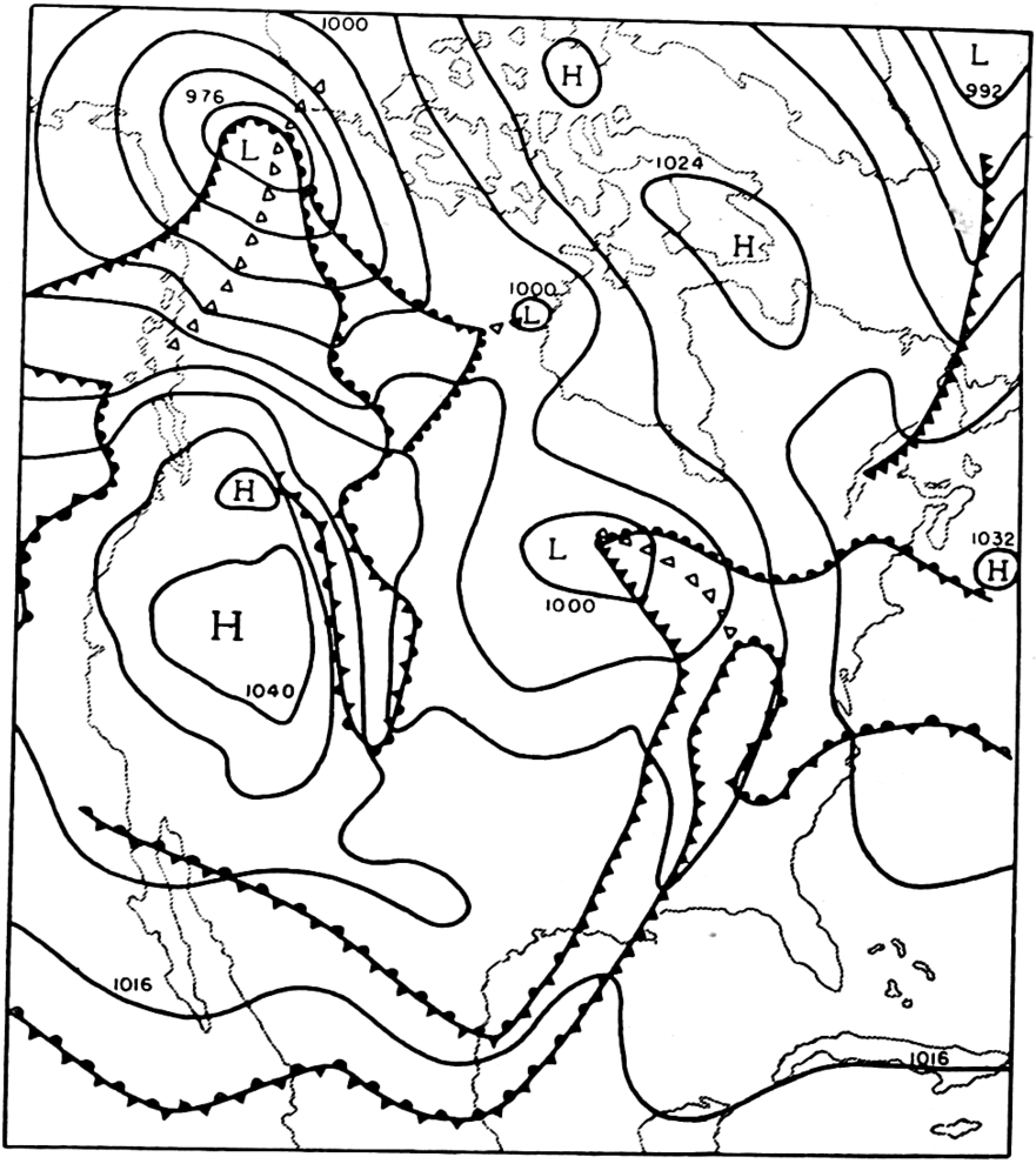


Figure 4. Surface synoptic chart — 5 January 1949, 1530 G.C.T.

determined. In a similar manner the Arctic front is not shown as continuous over the western sector of the chart, chiefly as the result of the large differences in surface temperature which are experienced between mountain stations, inland valley stations, and coastal stations, the Arctic air itself being confined chiefly to the valleys and the inland plateau region.

Some tephigram curves illustrating the various air masses involved are shown in Fig. 5. Isotherms are constructed at intervals of 20°C and isobars for the standard pressure levels of 1,000, 850, 700, 500, 400, 300, and 200 mb. Curve (a) is for

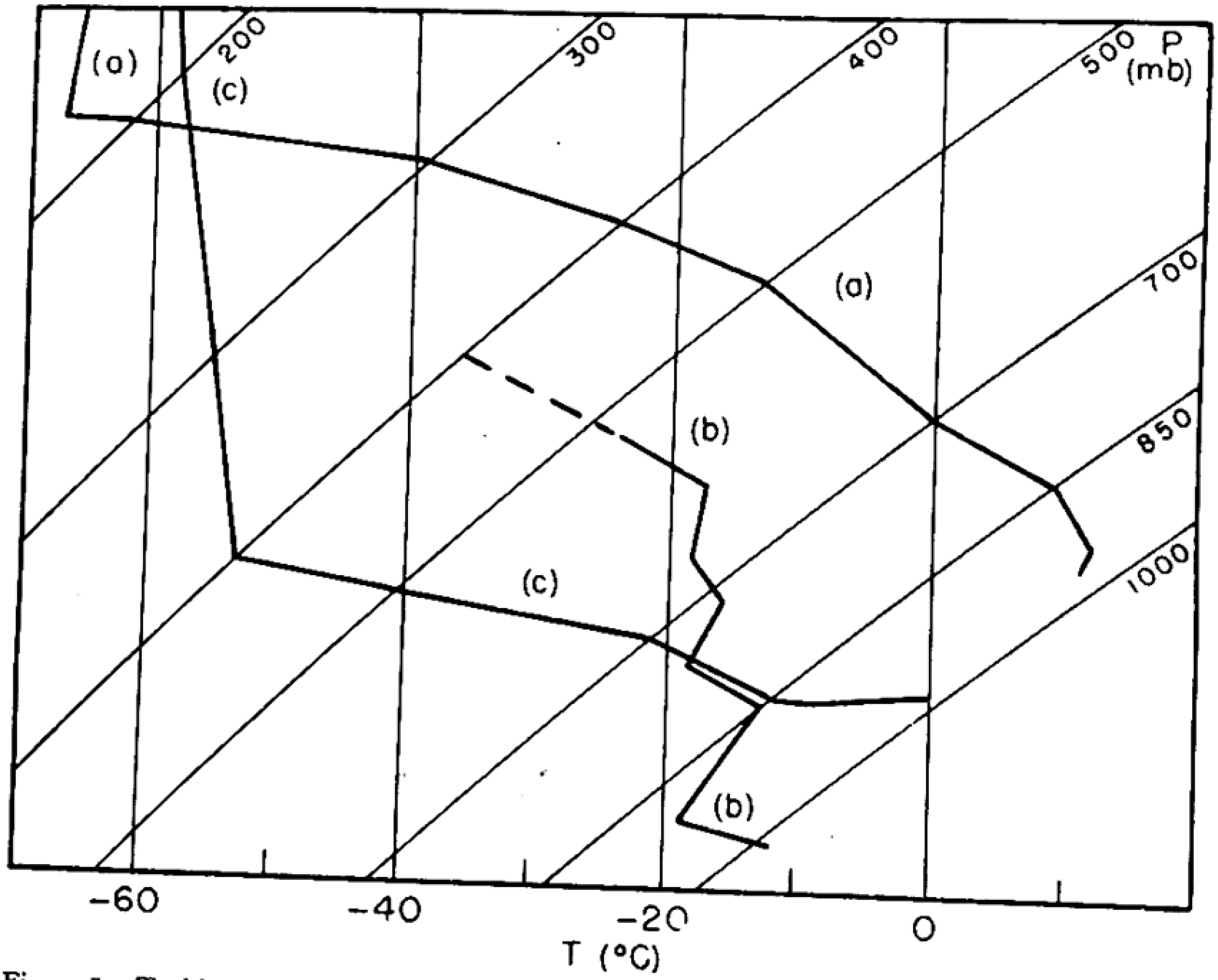


Figure 5. Tephigram curves, 5 January 1949, 1530 G.C.T. (a) Pittsburgh, Pa., (b) Goose Bay, Labrador, (c) 62°N. 33°W.

Pittsburgh, Pa., (just south of Lake Erie) and shows tropical air at all levels with a tropopause at 182 mb (approximately 42,000 ft). Curve (c) is for the ship station at 62°N. 33°W. (between Iceland and the southern tip of Greenland) and shows maritime Arctic air at all levels with a tropopause at 400 mb (approximately 21,000 ft). Curve (b) is for Goose Bay (in Southern Labrador) and shows four more or less distinct air masses. The ascent ceased at 520 mb but the curve has been extrapolated to 400 mb for comparison purposes. The air below 905 mb is continental Arctic, which has undergone slight modification. From 850 to 745 mb the air is maritime Arctic ; and from 720 to 655 mb. the air is probably cold maritime Polar. A clear-cut frontal surface at 610 mb marks the presence at and above this level of warm maritime Polar air. All three fronts show excellent lapse-rate discontinuities and, unless extremely shallow, are of dynamic significance as well as synoptic significance.

Over the area represented in Fig. 4 there were available for analysis approximately 90 tephigrams and 135 hodographs. From the analysis of these data there were constructed four sets of frontal contours (T-wmP, wmP-cmP, cmP-mA, and mA-cA), only two of which appeared to be of major significance in the over-all development pattern. Fig. 6 shows the frontal contours, from 1,000 to 400 mb,

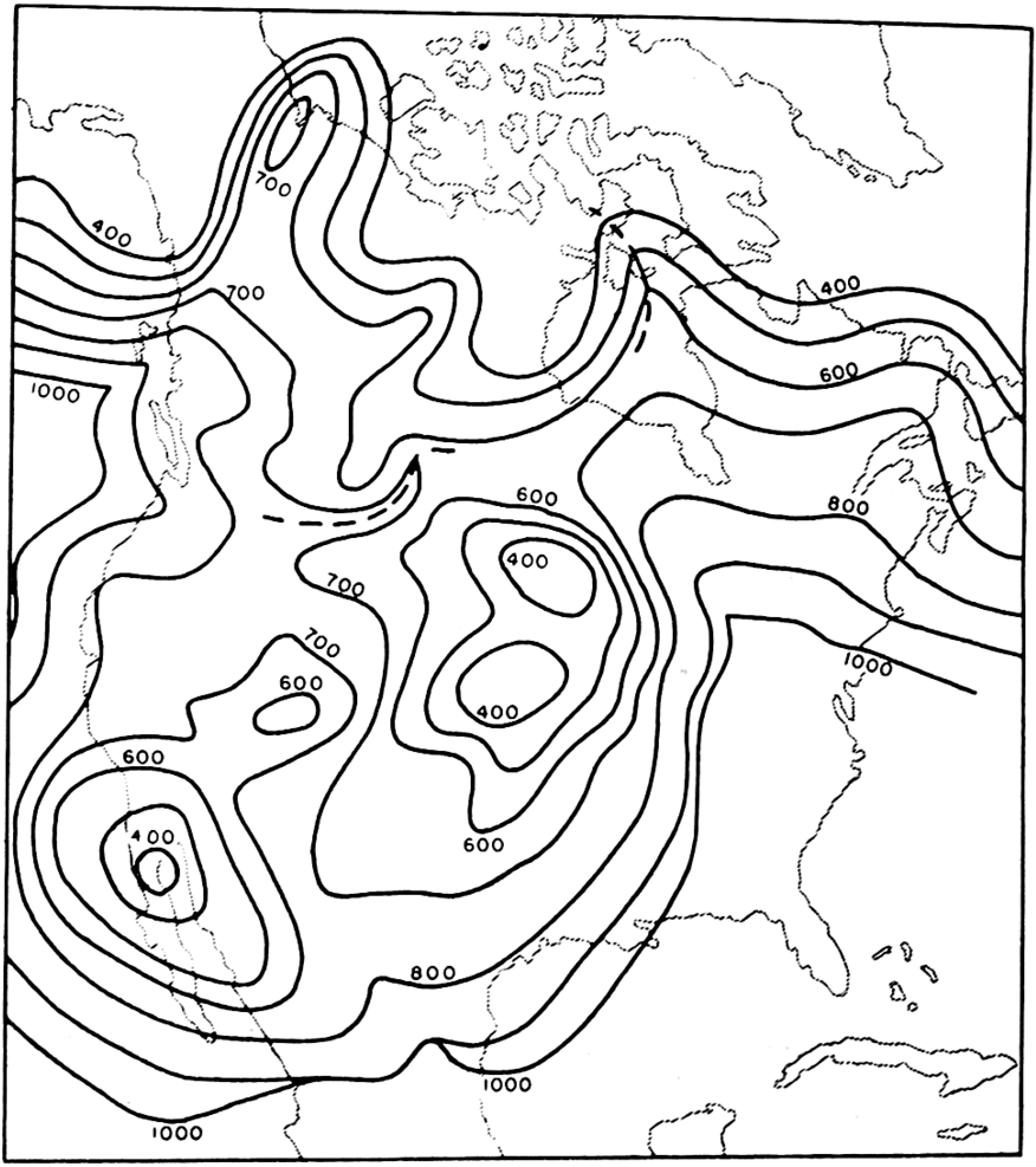


Figure 6. Frontal contours for warm maritime Polar air, 5 January 1949, 1530 G.C.T. (1,000 to 400 mb)

for warm maritime Polar air. The lines thus represent the intersection of the Polar front with the various isobaric surfaces as designated. These contours demonstrate clearly the presence and the structure of the two upper cold fronts shown on the surface chart of Fig. 4. No upper warm front is shown as yet but the appearance of the narrow valley of warm air in the Colorado region suggests that one should soon form just to the east of the Continental Divide. Later charts justify this hypothesis and in 24-36 hr the warm air descends almost to the surface in this sector, colder air remaining in the lower levels to the west. The frontal contours show several semi-isolated cold domes in the United States and one warm pocket in the Mackenzie River Valley in the Yukon. It is interesting to note that the cold dome west of Lake Superior is almost coincident with the surface low in that sector, which must represent, therefore, a cold quasi-vertical vortex. The cold dome south of California and Arizona also appears on the 500-mb chart as a closed low-pressure area. This chart affords an excellent demonstration of the fact that from the surface frontal pattern alone it is frequently impossible to deduce frontal positions at higher levels. These frontal contours show that at upper levels one minor wave and one major wave are moving eastward to the north of the assembly of cold domes. The major wave, over northern Hudson Bay, shows a definite pressure tendency discontinuity on the surface chart. On the previous day this wave had been associated with a long upper cold front, oriented roughly north-east to south-west. Existing sectors of that upper cold front are drawn on the frontal contour chart as dashed lines. It can be seen that a sector through Washington and Idaho has ceased to be significant, that the narrow tongue of warm air in Southern Saskatchewan is rapidly being pinched off against the slow-moving cold dome to the south-east, and that only the far northern sector of the upper cold front has remained significant, a development comparable to that represented on Fig. 1 (c). It was this upper cold front that initiated the cyclonic development visible on the surface chart to the west of Hudson Bay. Now that the major upper wave has outstripped the surface low, development of the latter should cease, and the low actually fills rapidly and disappears in the next nine hours.

Fig. 7 shows the frontal contours for cold maritime Polar air, *i.e.*, the positions of the Arctic front at various pressure levels. These contours substantiate the double-wave structure of the two major cyclonic depressions. It appears that the upper cold front mentioned in the closing sentences of the paragraph above also generated a weak wave on the Arctic front. As a result of the present out-of-phase relation no development of this wave on the Arctic front would be anticipated. Over the Western United States Arctic air is observed in the valleys (producing minimum temperatures in many sectors as low as —20°F) but not on the higher ground ; a unique set of frontal contours would have little significance here. A cold dome of Arctic air extends to above 500 mb west of the Great Lakes, producing a double dome structure in this area.

The frontal contours for Tropical air and maritime Arctic air are not reproduced here since in most sectors they did not appear to be significant above 600 mb. An exception to this occurred in the New Mexico region to the north-west of the Gulf of Mexico where two stations exhibited very well-defined Tropical frontal surfaces in the vicinity of 300 mb. The inter-Arctic contours showed a dome of continental Arctic air over northern Baffin Land extending to just above 600 mb on this occasion. The frontal surface was as usual extremely shallow in all directions from this dome.

4. THE MERITS OF A COMPLETE THREE-DIMENSIONAL ANALYSIS OF THE ATMOSPHERE'S STRUCTURE

Weather systems over North America, and doubtless elsewhere, are admittedly complex ; nevertheless this complexity makes the analysis of the atmosphere's

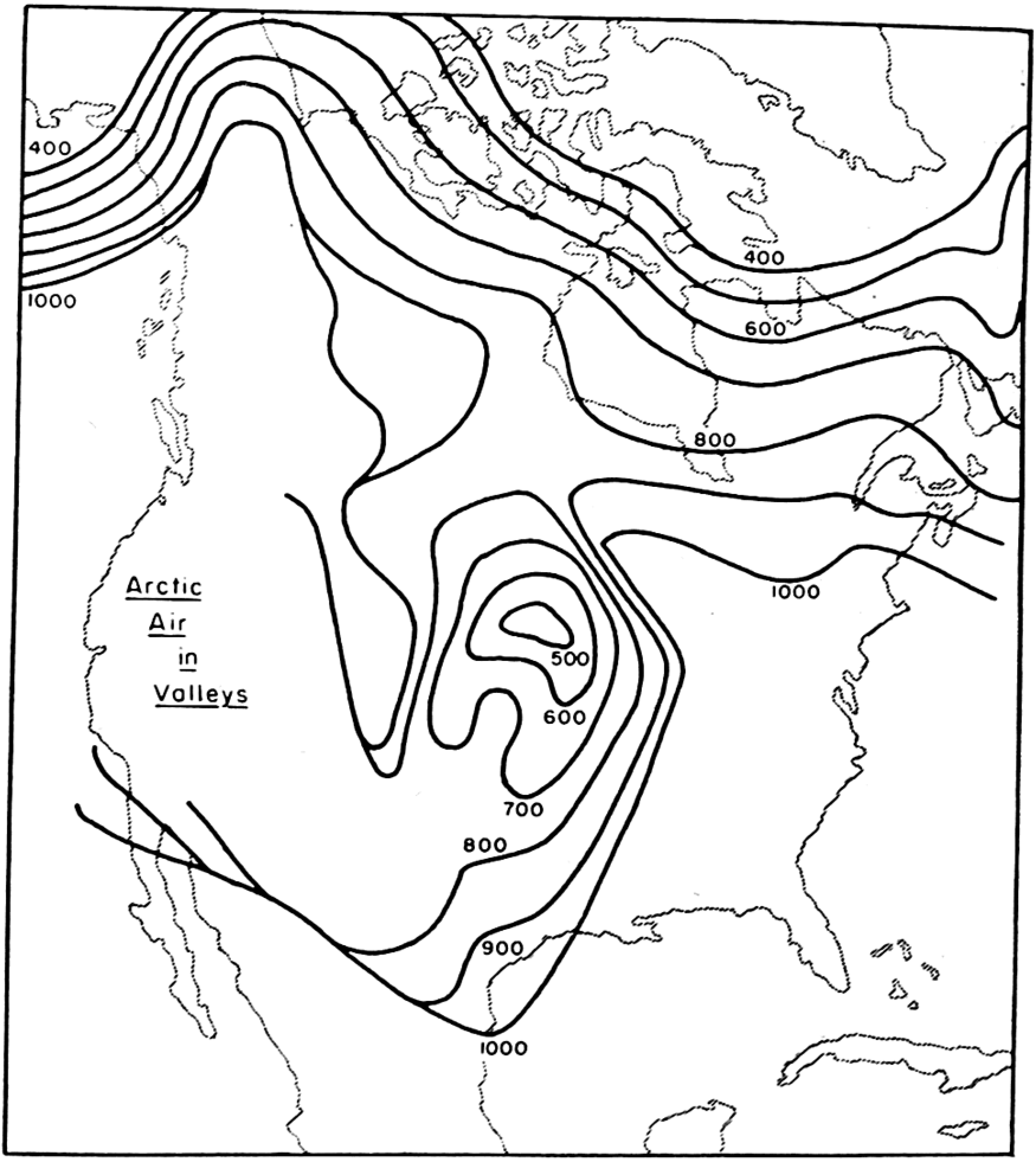


Figure 7. Frontal contours for cold maritime Polar air, 5 January 1949, 1530 G.C.T. (1,000 to 400 mb)

structure both a logical and a necessary starting-point of the forecast routine. Continuity of surface map analysis can best be obtained by a three-dimensional analysis of air mass and frontal structure. Such an analysis, moreover, can be performed in a logical and objective manner such that different analysts would agree except for minor details. However, no surface map analysis can represent the upper air structure completely and the surface map analysis should not be considered in any way a substitute for upper air analyses and upper air charts.

Thus the prognostician may postulate a three-dimensional continuity of atmospheric structure as an aid in forecasting development without making it necessary for the surface map analyst to represent the same on his surface map. It is of course necessary that the surface map be consistent with the upper air structure but upper fronts or fronts which are weak at the surface which do not constitute a surface weather discontinuity should, in general, not be represented on the surface map.

Without a complete three-dimensional picture of the atmosphere the preparation of prognostic charts as well as current charts is rendered more difficult. There are two methods available for the integration of synoptic data at various levels — thickness analyses and frontal contour analyses. It must be emphasized that these two techniques reinforce and supplement one another, neither is an adequate substitute for the other. In many respects a set of frontal contours will resemble the total thickness lines for the 500-mb surface — *e.g.*, where the frontal surface is high the thickness value will be relatively small. A frontal contour chart, however, does not embody merely the present pressure levels of frontal surfaces ; during its analysis full consideration must be given to vertical motions and a qualitative assessment of such motions is undeniably assisted by the air mass concept (their quantitative estimation will be mentioned later). Thus the regions of air mass transformations at upper levels can be recognized as zones of well-developed vertical currents. When this information is added to the horizontal mass transport, as obtained from thickness analyses, developments in the field of pressure may logically be deduced.

Additional information of prognostic value may be obtained from the frontal contour chart, when considered in conjunction with upper level charts. The forecaster is assisted in interpreting the present and probable future developments of upper cold and warm fronts and of cold domes and warm pockets ; in general the forecasting of frontal positions more than a day in advance will be facilitated.

A three-dimensional temperature and humidity analysis plus a dynamic analysis of air motion at all levels in the troposphere are required in order to explain adequately the weather processes associated with the current atmospheric structure and in order to arrive at the expected weather accompanying the forecast atmospheric state. The vertical velocity, one of the major factors responsible for weather, may readily be computed at the upper and lower boundary surfaces of all frontal transition zones from the Gião-Petterssen equation (Gião 1929) :

$$w = (u - u_F) \tan \theta,$$

where w signifies the vertical velocity, u the horizontal wind component normal to the front, u_F the speed of the front and $\tan \theta$ the slope of the front. The latter two quantities may be obtained from frontal contour charts. Using such vertical velocities the development and dissipation of cloud and precipitation areas can be forecast at least qualitatively, and deductions made concerning the possible release of potential instability.

Furthermore, if positions of fronts are entered on prognostic upper level charts a rough consistency check is provided between the anticipated weather and the vertical velocities deducible from the above relation.

In addition, frontal positions on upper air charts make possible the more accurate construction of thickness lines, height contours, and isotherms. Their utilization delimits the zones of maximum baroclinicity and maximum horizontal temperature advection in a manner virtually impossible from an *ab initio* analysis. In essence, their use enables the analyses of quasi-continuous vertical distributions of temperature and wind at discrete stations to be translated into corresponding quasi-continuous horizontal distributions of these elements in the vicinity of fronts. Recent studies (*e.g.*, Sutcliffe 1947) have indicated the prognostic importance of certain dynamic computations based on upper level charts which of necessity require these charts to be constructed as accurately as possible. This is certainly true of dynamic instability calculations (Van Mieghem 1946) and especially so in the vicinity of fronts, where the transition from strong stability to strong instability may take place over a distance of 300 mi or less. For such reasons and others already mentioned it is essential that a maximum amount of information be extracted from upper air data, particularly where such data are scanty ; the complete three-dimensional analysis of the frontal and air mass structure of the atmosphere may thus be seen to constitute a valuable part of this extraction process.

Acknowledgments

The author wishes to express his indebtedness to his colleagues in the Training and Research Section, Meteorological Division, for their helpful comments and discussions.

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551.515.11

An example of cyclogenesis in relation to Sutcliffe's theory of development

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Some 3 years ago Dr. R. C. Sutcliffe (1947) proposed certain expressions as a measure of the cyclonic development to be expected in any synoptic situation. Numerical evaluation of these expressions has now been made in a number of cases and is illustrated in Figs. 2, 3 and 4. It is also appropriate to consider the relation of Sutcliffe's treatment to the theory developed in America of an essentially barotropic atmosphere.

Both treatments can be derived from the familiar vorticity equation (see for example Rossby, 1940)

$$\mathrm{Div}_2 V = -\frac{1}{\zeta + l}\frac{d}{dt}(\zeta + l) \qquad . \qquad . \qquad . \qquad (1)$$

where $\mathrm{Div}_2 V$ is the horizontal divergence of the velocity

$$\zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} = \text{vertical component of the relative vorticity,}$$

l is the Coriolis parameter and d/dt denotes differentiation following the fluid motion.

Rossby's theoretical formula for the motion of the " long waves " in a barotropic atmosphere may be derived by putting $\mathrm{Div}_2 V = -\frac{1}{p_0}\frac{\partial p_0}{\partial t}$, where p_0 is the surface pressure, obtaining

$$\frac{d}{dt}\left(\frac{\zeta + l}{p_0}\right) = 0 \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

In recent papers Charney (1949a and b) justifies these results for a baroclinic atmosphere also by integrating Eq. (1) with respect to pressure from the top to bottom of the troposphere, and making use of the fact that the troposphere as a whole does not usually vary greatly in its depth. Eq. (2) then applies approximately to a level in mid-troposphere, the " equivalent barotropic level."

Charney makes special allowance for the effect of large-scale topography and proceeds to make some broadly successful numerical calculations of the changes in troposphere flow to be expected from some observed initial situations.

Sutcliffe, on the other hand, considers the difference between $\mathrm{Div}_2 V$ as given by Eq. (1) at two levels, best considered as at the top and bottom of the troposphere. On certain plausible assumptions he derives the following convenient expression for the relative divergence.

$$\mathrm{Div}_p V - \mathrm{Div}_p V_0 = -\frac{1}{l}\left\{ V'\frac{\partial l}{\partial s} + V'\frac{\partial \zeta'}{\partial s} + 2V'\frac{\partial \zeta_0}{\partial s} \right\} \qquad . \qquad . \qquad (3)$$

where Div_p is the divergence evaluated in an isobaric surface (differing only by a negligible amount from Div_2), where V and V_0 are wind vectors at the upper and lower pressure levels and where V' and ξ' are the thermal wind vector between these levels and its vorticity. $\partial/\partial s$ denotes differentiation in the direction of the thermal wind.

The relative divergence is of great meteorological significance because of its direct connection with the vertical velocity and hence with cloud and precipitation; however in practice Dr. Sutcliffe's results have been applied more commonly up to now to assess cyclogenesis and anticyclogenesis. In doing this we are in fact using the result in an intermediate form.

I do not wish to go into the assumptions of Charney's and Sutcliffe's treatments but I see nothing incompatible in them. Sutcliffe makes use of the observed fact that the thermal wind direction is approximately constant with height but Charney makes the more restrictive assumption of the same wind direction at all heights.

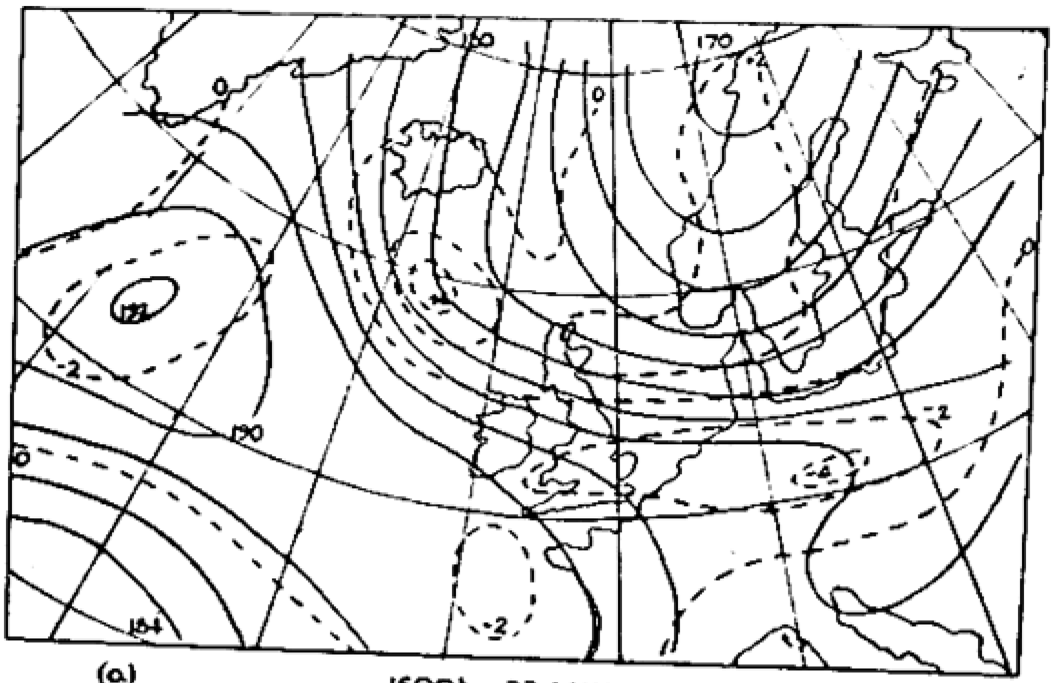
By considering one or two possible models of atmospheric systems we can see that the two treatments describe two essentially different and independent aspects of the flow. If we consider first an atmosphere with negligible horizontal temperature gradients, the barotropic model, a rough first approximation to the solution of Eq. (2) is obtained by regarding p_0 as constant because the percentage changes in pressure are normally small compared with the changes of vorticity. The equation then indicates that absolute vorticity is advected with the wind. (Charney's solution is however a better approximation than this.) Thus we can see that in a barotropic atmosphere we should observe local changes in vorticity at all levels as vorticity is advected with the stream, but there is no " development " in the sense in which Sutcliffe uses the word, no relative divergence and no vertical motion because V' is zero in (3). The atmosphere probably behaves more or less in this way in association with old cyclonic systems where horizontal thermal gradients are weak.

It is also possible to conceive of an atmospheric system in which the flow in the equivalent barotropic atmosphere would not change but in which there would be development and relative divergence as studied by Sutcliffe. One such mode would consist of a stationary long wave in the equivalent barotropic atmosphere of wave-length given by the Rossby formula and with flow zero at the ground increasing toward the tropopause where it should be approximately twice the velocity of the " equivalent barotropic atmosphere."

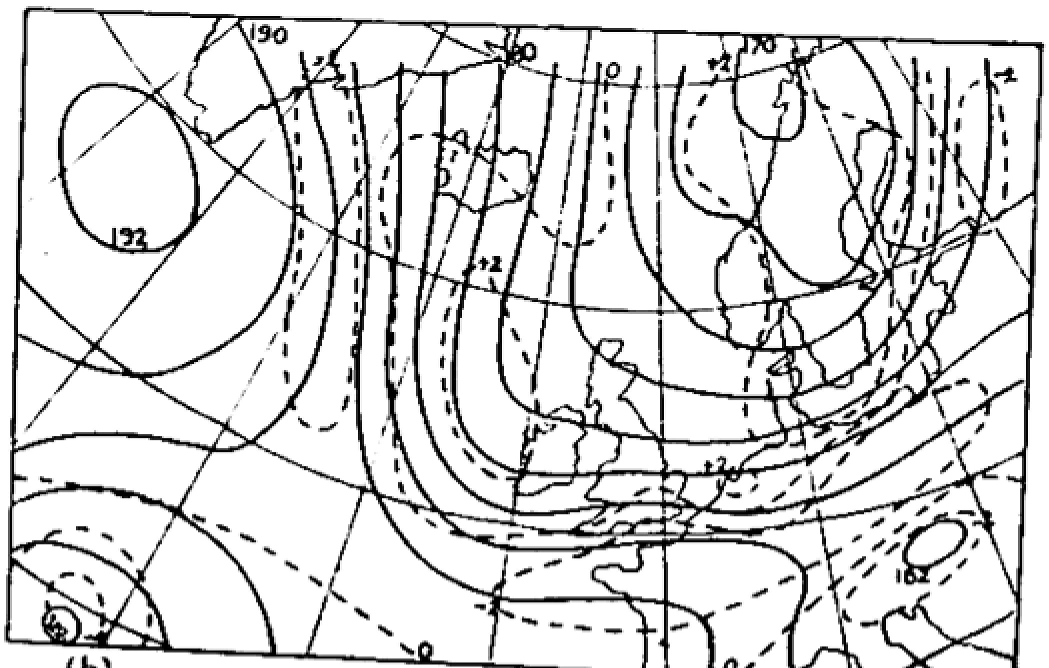
In practice we find that in the early stages of cyclogenesis the surface flow is often weak, and the thickness lines and contours at 500 mb have similar configurations. Under these circumstances the term $V'\partial\zeta'/\partial s$ in (3) contributes to cyclonic development in those areas into which cyclonic vorticity is being advected in the equivalent barotropic atmosphere.

This was true over the British Isles on 22 and 23 May 1948 when a wave depression developed.

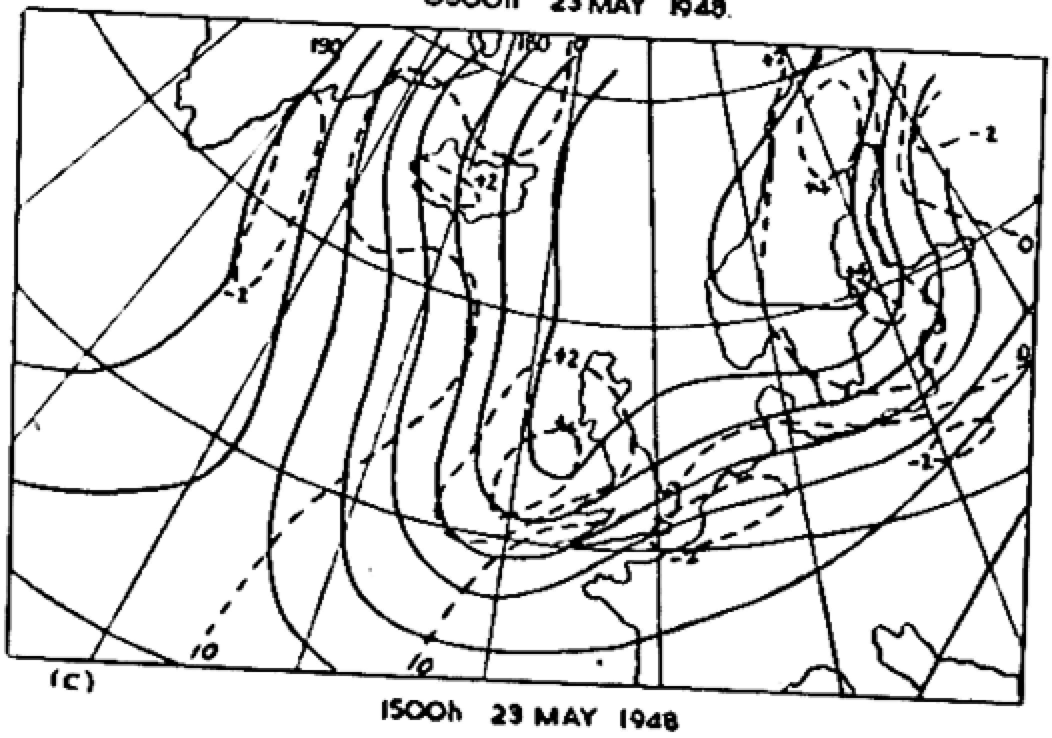
Figs. 1 (a), (b), (c) show the contours of the 500-mb surface at 12-hour intervals and superimposed on them isopleths of relative vorticity computed on the assumption of geostrophic motion. The absolute vorticity would show essentially the same pattern (with different numerical values) and I have not made any allowance for variation of the Coriolis parameter with latitude in any of these computations because its effect was clearly small.



(a) 1500h 22 MAY 1948



(b) 0300h 23 MAY 1948.



(c) 1500h 23 MAY 1948

Figure 1. Contours of the 500-mb surface (continuous lines at 100-feet intervals) and isopleths of relative vorticity (dashed lines — units $hr^{-1} \times 10^{-1}$)

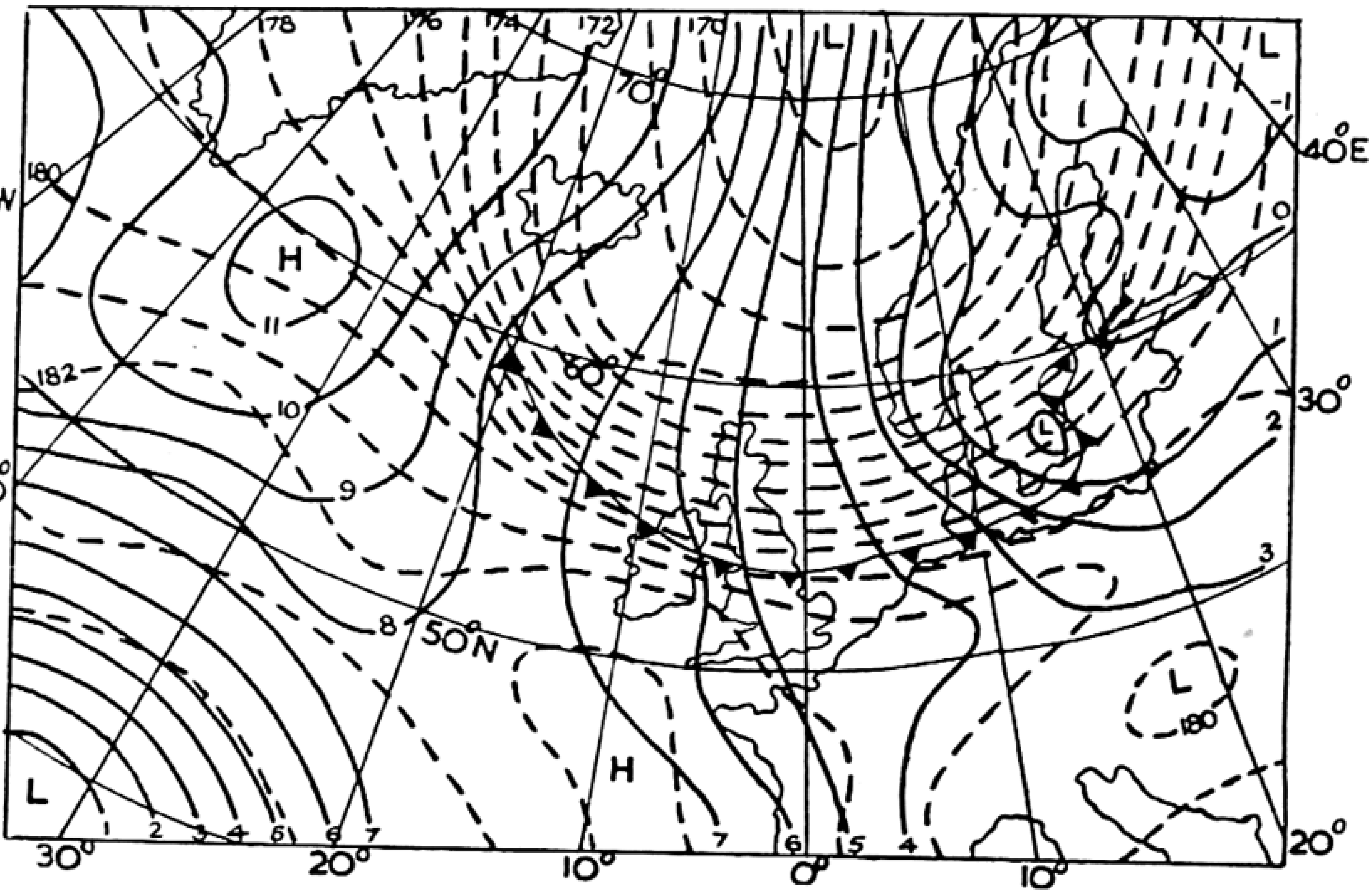


Figure 2 (a). Contours of 1000-mb surface and thickness lines 1000-500 mb at 100-ft intervals. 1500 G.M.T., 22 May 1948.

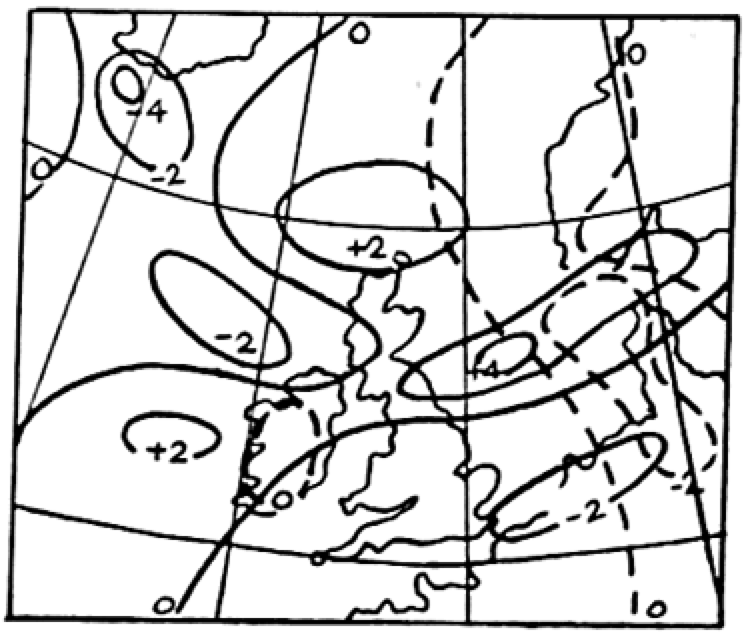


Figure 2 (b). Isopleths of thermal vorticity term and thermal steering term.

Thermal steering term $-2V'\frac{\partial\zeta_0}{\partial s}$ (dashed line)

Thermal vorticity term $-V'\frac{\partial\zeta'}{\partial s}$ (continuous line)

(Units hr^{-2})

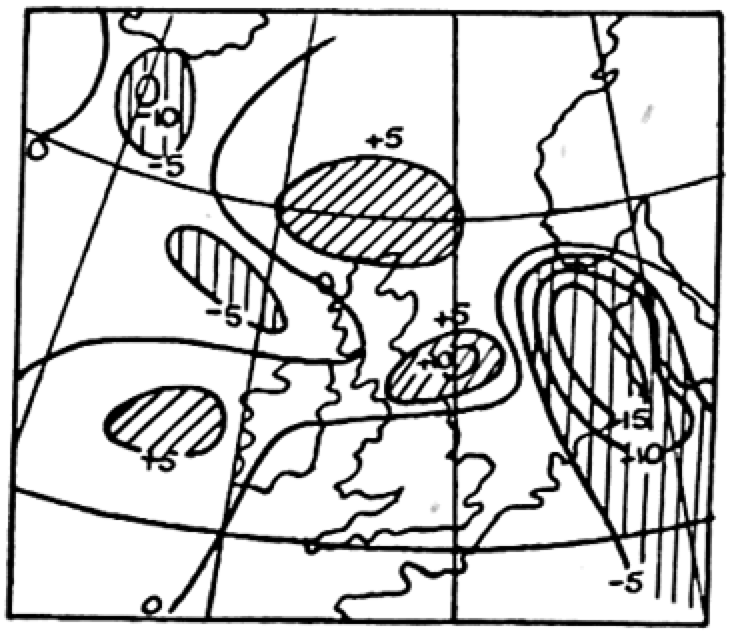


Figure 2 (c). Isopleths of Sutcliffe's expression of the relative divergence.

$$-\frac{1}{l}\left(V'\frac{\partial\zeta'}{\partial s}+2V'\frac{\partial\zeta_0}{\partial s}\right)$$

(Units hr^{-1} × 10^{-2})

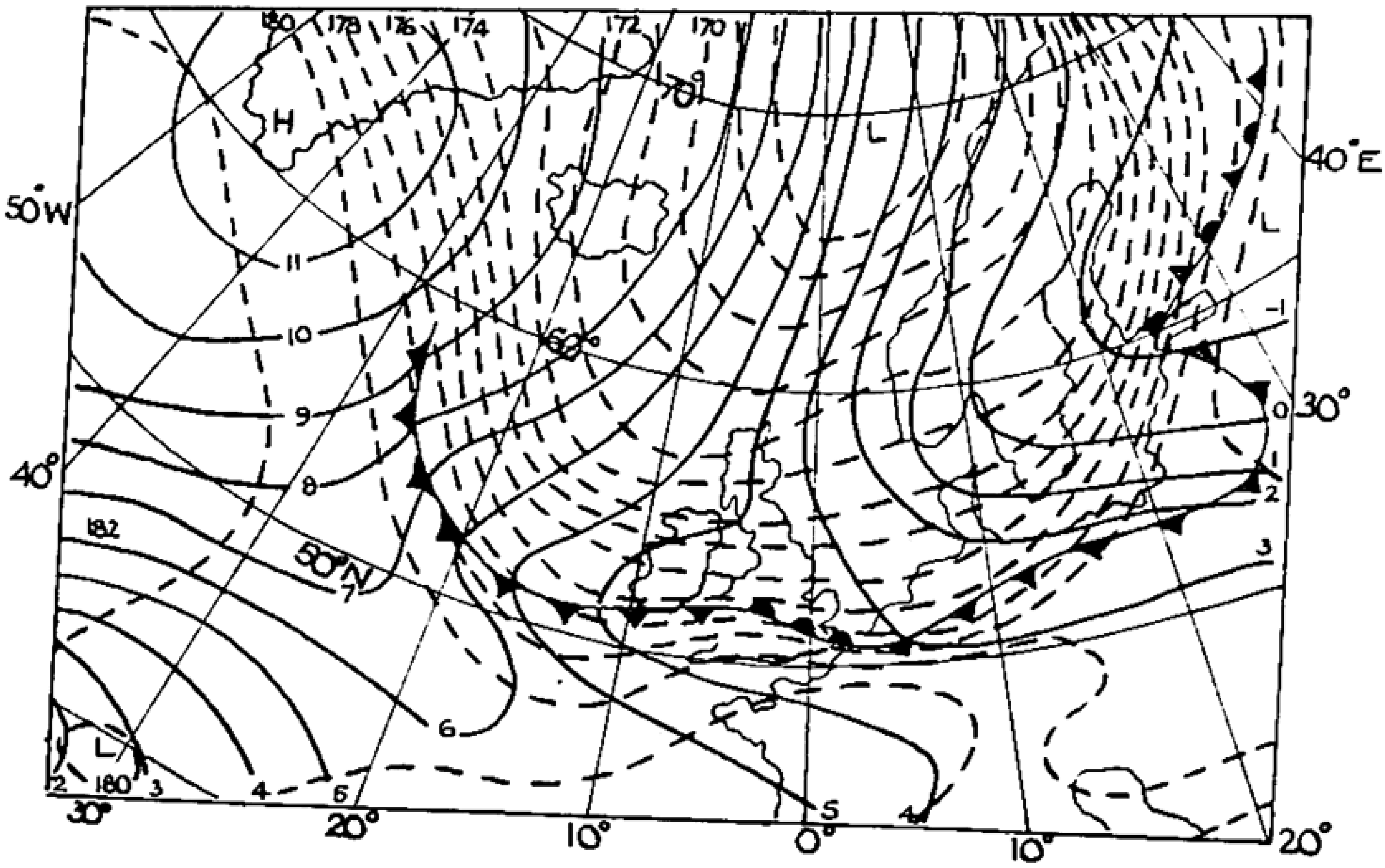


Figure 3 (a). Contours of 1000-mb surface and thickness lines 1000-500 mb at 100-ft intervals. 0300 G.M.T., 23 May 1948

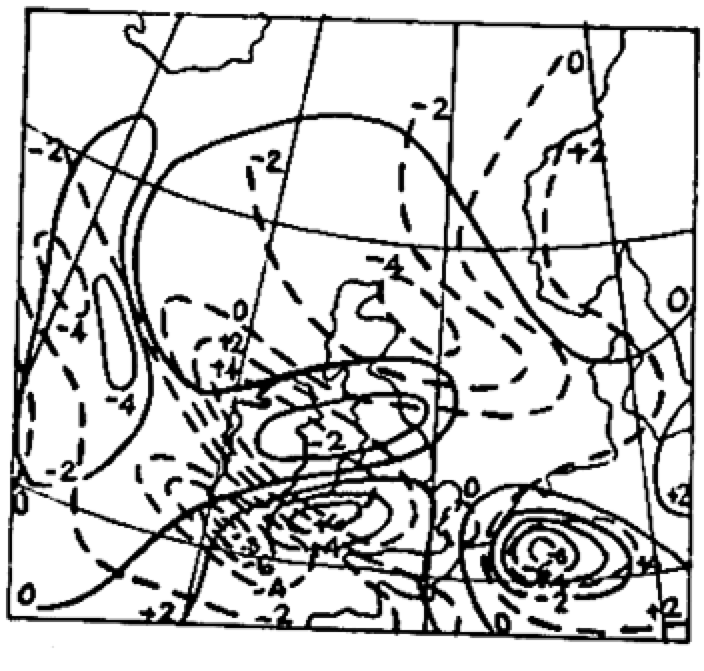


Figure 3 (b). Isopleths of thermal vorticity term and thermal steering term.

Thermal steering term $-2V'\frac{\partial\zeta_0}{\partial s}$ (dashed line)

Thermal vorticity term $-V'\frac{\partial\zeta'}{\partial s}$ (continuous line)

(Units hr^{-2})

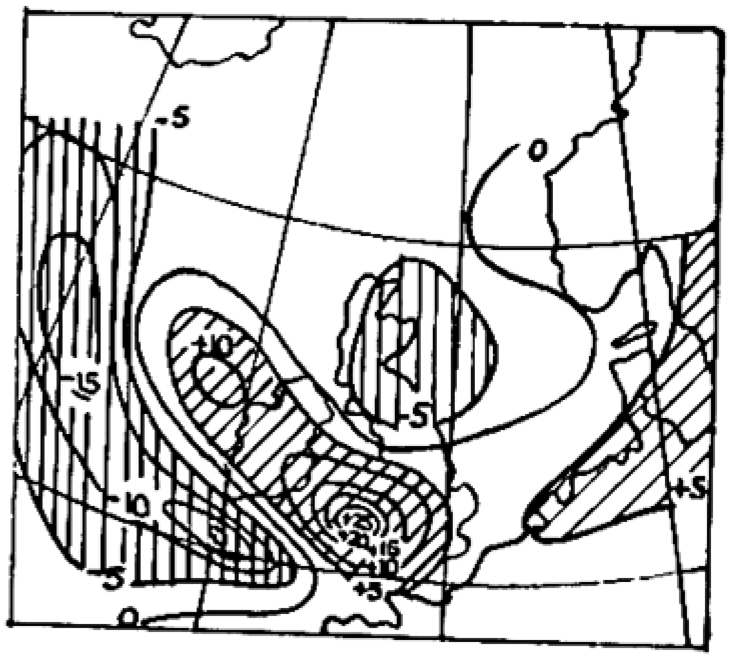


Figure 3 (c). Isopleths of Sutcliffe's expression of the relative divergence.

$$-\frac{1}{l}\left(V'\frac{\partial\zeta'}{\partial s}+2V'\frac{\partial\zeta_0}{\partial s}\right)$$

(Units hr^{-1} × 10^{-2})

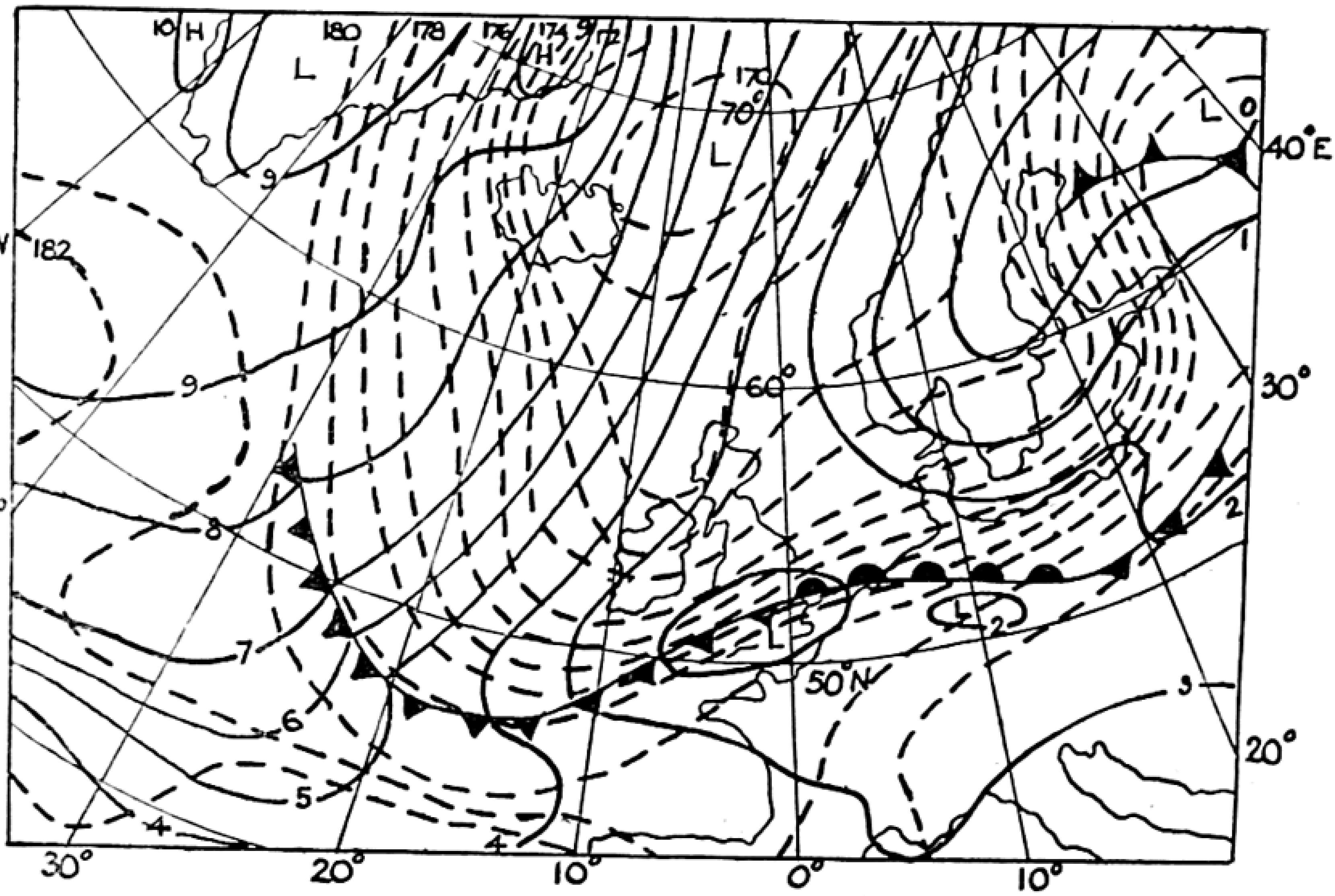


Figure 4 (a). Contours of 1000-mb surface and thickness lines 1000-500 mb at 100-ft intervals. 2500 G.M.T., 23 May 1948

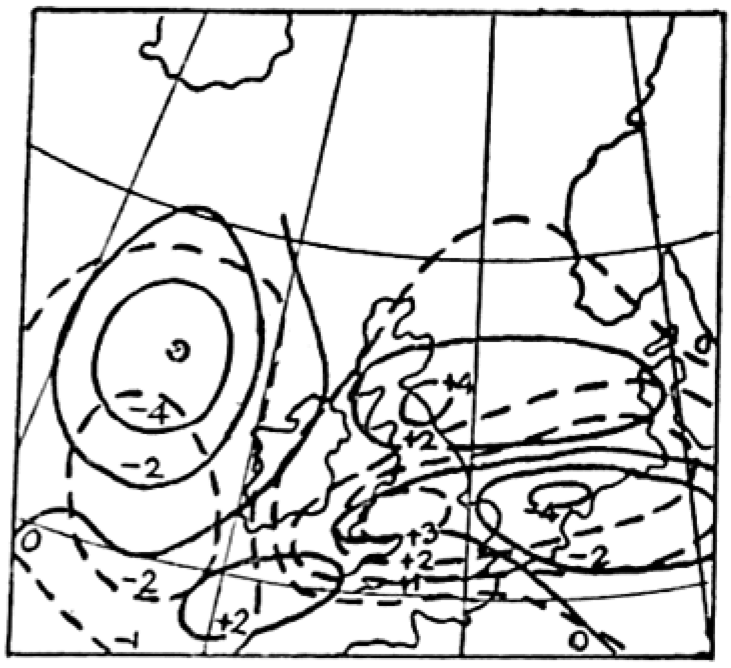


Figure 4 (b). Isopleths of thermal vorticity term and thermal steering term.

Thermal steering term $-2V'\frac{\partial\zeta_0}{\partial s}$ (dashed line)

Thermal vorticity term $-V'\frac{\partial\zeta'}{\partial s}$ (continuous line)

(Units hr^{-2})

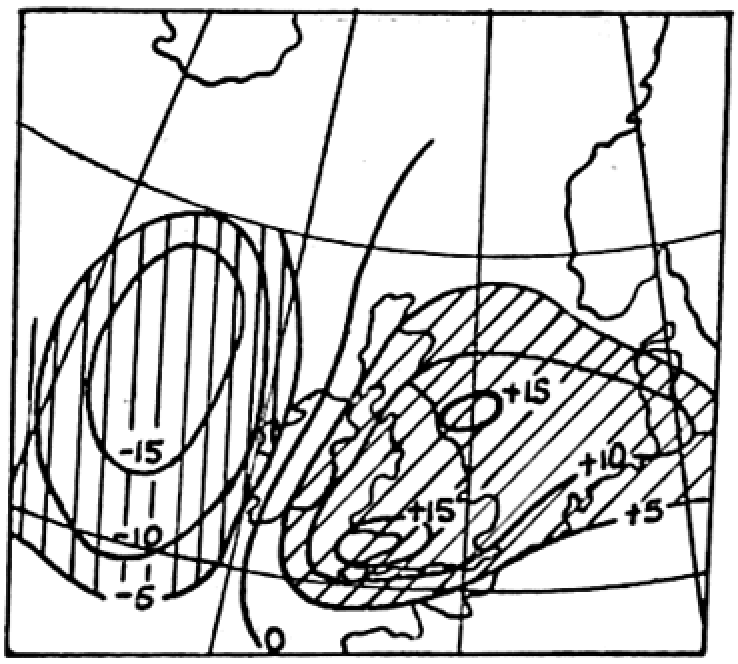


Figure 4 (c). Isopleths of Sutcliffe's expression of the relative divergence.

$$-\frac{1}{l}\left(V'\frac{\partial\zeta'}{\partial s}+2V'\frac{\partial\zeta_0}{\partial s}\right)$$

(Units $\text{hr}^{-1}\times 10^{-2}$)

From Fig. 1 we note that the maximum of vorticity associated with the trough line moved south-east approximately with the speed of the wind, and the movement of other features of the vorticity pattern is broadly consistent with the advection of vorticity, showing that the 500-mb flow behaved similarly to the theoretical equivalent barotropic atmosphere — whether the departures from simple advection are in accord with Eq. (2) I have not attempted to verify.

Figs. 2, 3 and 4 show the thickness pattern superimposed on the contours of the 1000-mb surface, together with an evaluation of the principle terms on the right-hand side of the Eq. (3), the development terms of Sutcliffe. The features which may be noted are :

1. the close association between the maxima of these terms and the area of cyclogenesis and
2. the increasing importance of the term $V'\partial\zeta_0/\partial s$, the so-called thermal steering term, as the system develops.

This term indicates a tendency for the system to move in the direction of the thermal wind and the growth of this terms accounts for the north-east motion of the system following an initial period of little movement in association with the maximum of the term $V'\partial\zeta'/\partial s$, the thermal development term, east of the thermal trough line.

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551.509.317 : 551.547

A note on the individual changes in the thickness of isobaric layers

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SUMMARY

An account is given of the prognosis of the thickness of an isobaric layer by non-advective and non-adiabatic extrapolations. This prognostic operation takes into account the changes occurring in the quasi-conservative aspects of thickness change. These non-advective changes in the thickness are due to (*a*) vertical motion, (*b*) condensation or evaporation, contact of the air with warmer or colder surfaces of the earth, and radiative cooling of the air. A synoptic interpretation of these atmospheric processes leads to an empirically developed generalization concerning non-advection. Finally, the prognostic value of this idealized model is shown briefly in the daily routine of upper-air map prognosis.

1. INTRODUCTION

The prognosis of the upper-air pressure field may proceed in the same way as its analysis. By the layer method, the thickness of an isobaric layer is first predicted and then graphically added to the height of the lower bounding surface of the layer in order to obtain, *a posteriori*, the height of the upper boundary. As a semi-Lagrangian method of predicting the advective change in the thickness, the thickness lines for time t_0 are considered as conservative mean isentropes of their layer embedded in its isobaric flow ; and, as the first stage in the order of operations for upper air prognosis, they are displaced passively with the estimated future flow of the same layer, for the period, say, from t_0 to $t_0 + 24$hr. As stage two, the layer prognosis is completed by supplementing the advective adiabatic extrapolation of the layer isentropes with physical considerations regarding the non-advective and non-adiabatic contributions to their future displacement.

2. THE PROGNOSIS OF THE THICKNESS BY NON-ADVECTIVE AND NON-ADIABATIC EXTRAPOLATION

We choose an appropriately idealized prognostic pattern of the non-advective change in the thickness for $(t_0 + 24\text{hr}) - t_0$. Whereas the advective-adiabatic extrapolation in stage one is mainly mechanical in its application, this prognosis on the other hand brings into play the creativeness and intuition of the forecaster and depends largely upon his experienced insight in perceiving and anticipating the non-advective (and non-adiabatic) processes in the atmosphere and in evaluating their effect upon thickness change. The rational separation of the prognosis into its mechanical and non-mechanical operations (*viz.* stages one and two) not only concentrates the forecaster's efforts on these more or less intangible processes and the associated physical extrapolations, but also provides for the acquisition of forecasting experience concerning such processes and extrapolations. Later verification will indicate the extent to which the non-advective, non-adiabatic processes have been correctly anticipated in the 24-hr prognosis : this verification is automatically accomplished at $t_0 + 24$hr, at which time the actual non-advective change of thickness for the period from t_0 to $t_0 + 24$hr is analyzed. The final 24-hr thickness prognosis is now obtained simply by adding graphically the prognosis of the advective thickness pattern for $t_0 + 24$hr and the prognostic pattern of the non-advective change in the thickness pattern for the period $(t_0 + 24\text{hr}) - t_0$.

In Fig. 1a, for example, it is seen that there is a pronounced centre of non-advective increase in the 1,000-500 mb thickness over the south-central United States, where the north-westerly flow is descending from the Plateau region. The cold front and occlusion in eastern United States is inactive, as evidenced by the precipitation and altostratus-nimbostratus cloud system. There is a ridge of non-advective increase in the 1,000-500 mb thickness to the rear of this front, the region traversed by this front during the past 24hr. There is a tongue of non-advective decrease in the 1,000-500 mb thickness along the western seaboard of Canada and Washington where there is forced ascent of the westerly flow in crossing the Coast Mountains and the Cascade Range. There is a pattern of non-advective decrease in the 1,000-500 mb thickness along the eastern seaboard of the United States and in Southern Quebec, where a saturated and conditionally stable warm air mass is being lifted in its poleward ascent over quasi stationary warm-front surfaces. It can be seen that the non-advective decrease in the 1,000-500 mb

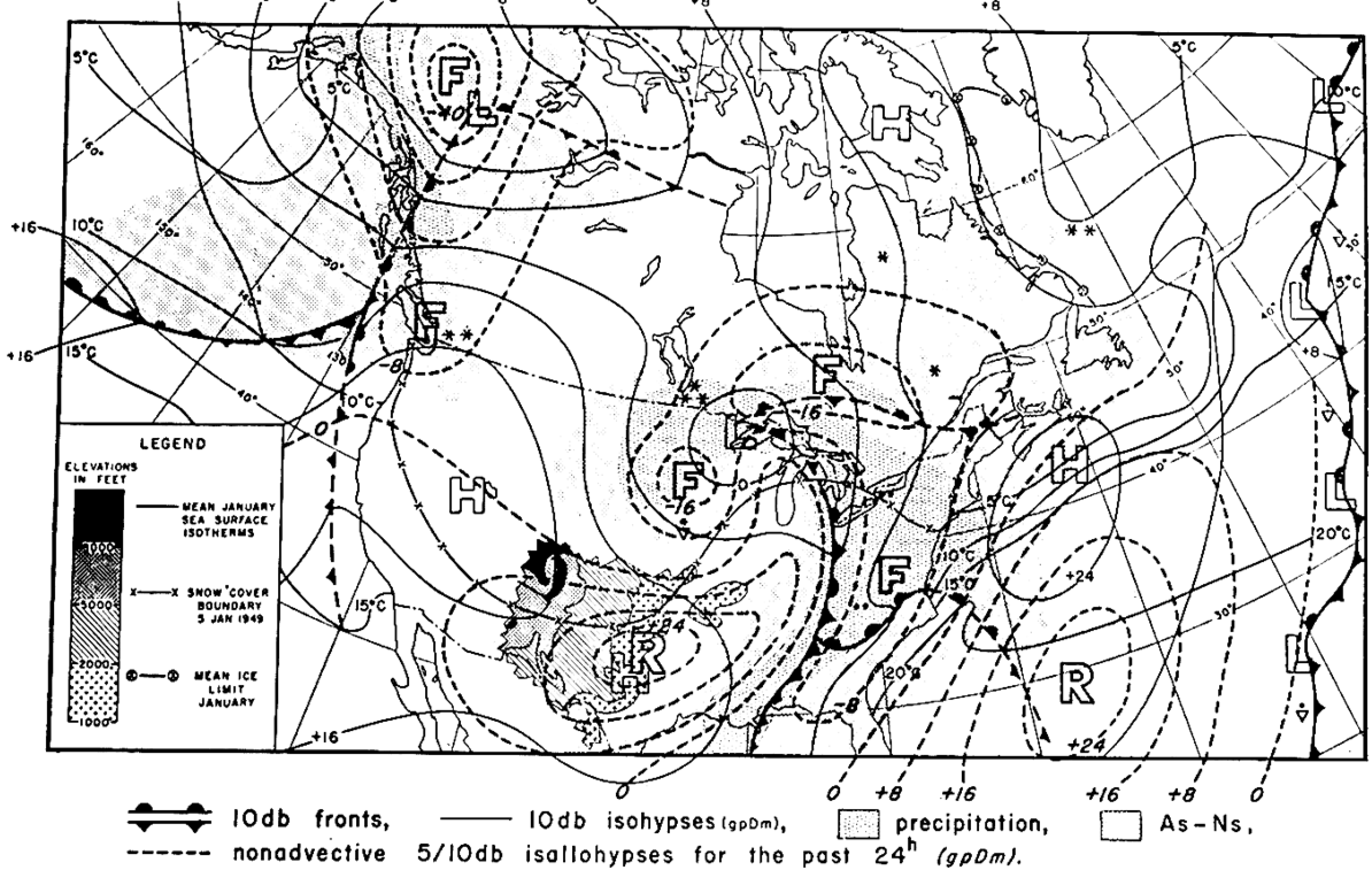
LEGEND
ELEVATIONS IN FEET
MEAN JANUARY SEA SURFACE ISOTHERMS
SNOW COVER BOUNDARY 5 JAN 1949
MEAN ICE LIMIT JANUARY
10db fronts,
10db isohypses (gpDm),
precipitation,
As-Ns,
nonadvective 5/10db isallohypses for the past 24^h (gpDm).

Figure 1(a)

Figures 1 (a) and (b). Maps for 15 hr G.M.T., 5 January 1949, showing the relation of the non-advective change in the 1,000-500 mb thickness for the past 24 hr, to (*a*) the 1,000 mb topography and 1,000-500 mb thickness, (*b*) the areas of precipitation and altostratus-nimbostratus, (*c*) the extent of snow-cover, (*d*) the sea surface temperature, and (*e*) the pertinent orography. This non-advective change in the thickness has been based on streamline maps at 6-hr intervals, and instantaneous values of the geostrophic advective change in the thickness computed from the statical-kinematical tendency equation. (Note 1 db = 100 mb.) This figure is taken from the *Compendium of Meteorology*, American Meteorological Society.

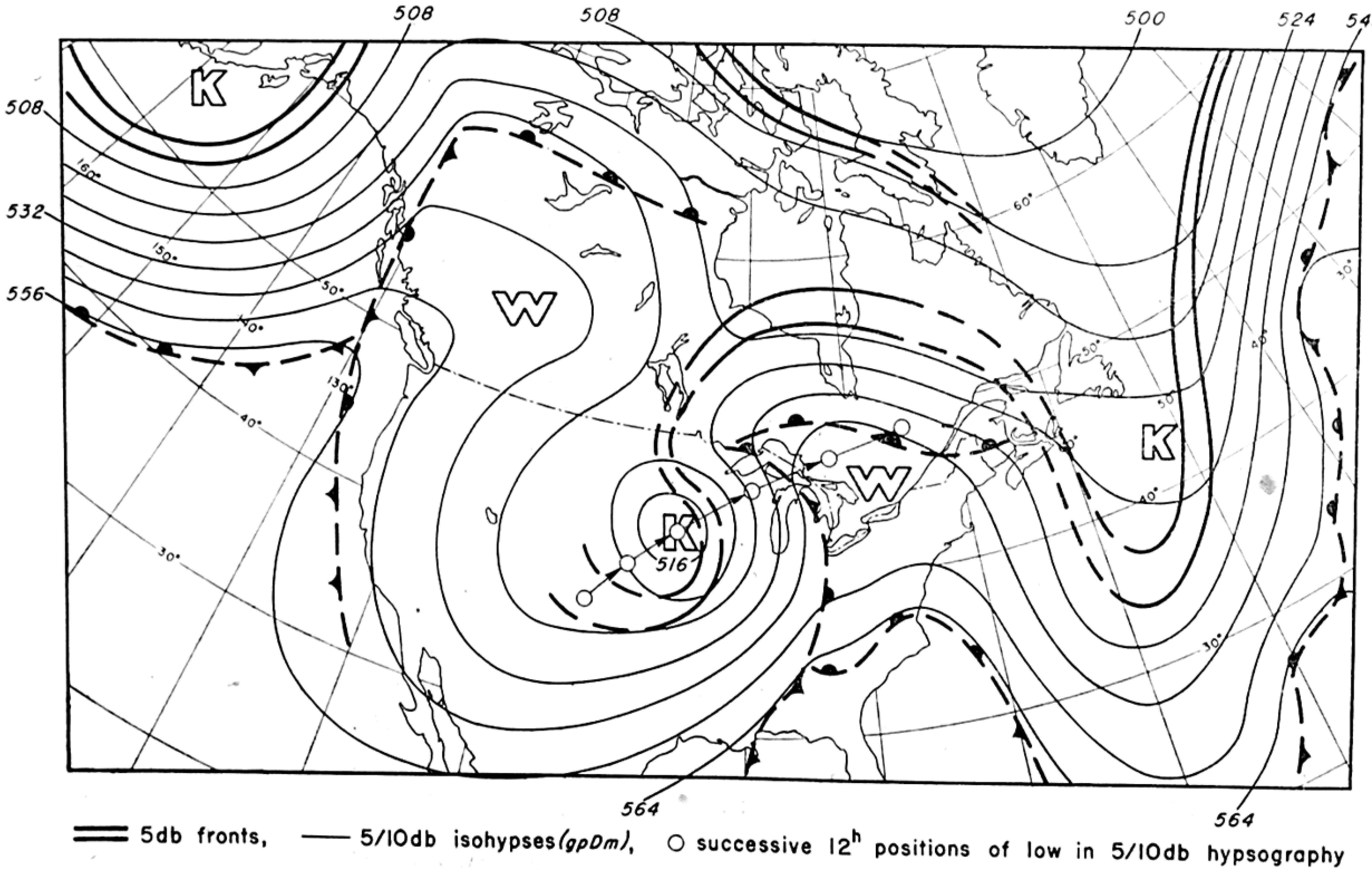

Figure 1(b). For additional explanation see legend below Fig. 1(a).

thickness corresponds roughly to the areas of precipitation and of altostratus-nimbostratus cloud system. Where the north-westerly flow of the cold air passes from the snow-covered Great Plains to the snow-free south-central States, there is shown to be a crescent-shaped centre of non-advective increase in the 1,000-500 mb thickness ; the opposite considerations, as it can be seen, apply to the southerly flow of warm air in passing from the snow-free south-eastern United States to snow-covered New England and Quebec.

The foregoing considerations of the non-advective changes in the thickness, substantiated by synoptic experience, can be generalized into a synoptic upper air model, which relates the non-advective changes in the thickness to the corresponding thickness pattern. For example, the centre R of non-advective increase in Fig. 1 is just behind the equatorward portions of the troughs in the 1,000-500 mb thickness pattern in Fig. 1b. The ridges in the thickness pattern (see Fig. 1b) and its centres of non-advective decrease, F in Fig. 1a, are similarly related. Generally speaking, these centres of non-advective increase and decrease in the thickness pattern for the 24-hr period appear respectively *at, or just behind*, the equatorward portion of the trough and the poleward part of the crest in the thickness pattern for t_0.

Discussion

Mr. A. G. Matthewman (*Meteorological Office*) : Dr. Godson has said that it is perfectly possible to analyse the fronts on a surface chart objectively. Dr. Bleeker has said that no synoptician will believe another synoptician's analysis. In my experience the T-ϕ gram curves often show complexities and blurring, rather than the clear-cut features of, *e.g.*, the four-air-mass structure of Dr. Godson's Fig. 5, curve (b). Moreover the singular points on the corresponding wind sounding may occur at levels different from the singular pionts on the (virtual) temperature curve. Yet a third curve, the condensation curve, which shows the characteristics of the air-masses involved, may have ambiguous or ill-defined singular points at levels other than the singular points of the temperature and wind curves. The effect is presumably due mainly to mixing — severe turbulence being sometimes reported by aircraft in frontal regions. It is not obvious why priority should be given to the temperature curve, since heat is equally involved in the mixing. The discrepant singularities mentioned probably account for some of the analysts' dilemmas.

With regard to Dr. Bleeker's emphasis on the role of differential heating in the deepening of cyclones, a case of 23-25 October 1949 examined in detail suggests that some deepening, 0(7) mb, at the centre of a polar wave north of the Azores occurred during the normal occlusion process and that afterwards, although cold air was being rapidly warmed in its circulation round the depression, slight filling of the (parent) centre occurred ; but perhaps some light is thrown on the maintenance of the rather sharp trough, apparently non-frontal in the normal sense, to south of the (parent) centre.

Dr. S. Petterssen : Several of the speakers have discussed the structure of developing systems and endeavoured to explain the development in terms of local parameters, leaving out of consideration the interaction between neighbouring systems. A brief discussion of the interaction between systems is given in Section 4 of my paper " Some aspects of the general circulation " which will be discussed under the symposium on the General Circulation. The results indicate that this interaction is very important for the development of certain types of systems.

Mr. J. M. Craddock (*Meteorological Office*) : I am investigating those changes in the thickness of the 1000-700 mb and 700-500 mb layers which contribute to changes in the large-scale thermal distribution in the troposphere. The thickness tendency for an isobaric layer can be divided into three components ; advective, dynamic and non-adiabatic. It is easy to show, by substituting typical values, that each is capable of being important. The advective component for the layers considered often exceeds the total tendency ; so does the dynamic component (due to vertical motion) in a developing anticyclone. The non-adiabatic component was investigated in one situation, when cold air was advected over warm sea. The cases considered were of arctic air crossing Iceland and later the Gulf Stream on its way to the British Isles, and the rate of heating could be very great, exceeding 1°F per hour at all levels from the surface to 700 mb, and extending at lower rates to above 500 mb. These air masses were gaining heat energy, cm^{-2}, at a rate exceeding half the solar constant and continued to do so for a period of 24 hours. This is probably the most rapid non-adiabatic heating to which any air mass could be subjected.

To find the part played by the advective component, six-hourly measurements were made of the total thickness tendency and of the advective component, for six months at Downham Market in East Anglia. These show that the best estimate of the total tendency is obtained by taking two-fifths to one-half of the advective component, but that the correlation of this estimate with the total tendency is only 0·5 to 0·6. The advective component was then compared with the non-advective component (*i.e.* the sum of the dynamic and the non-adiabatic components) and a correlation of —0·7 was found between them. This occurred in each of the six winter months considered so there is no doubt of its reality. These results show that there is no prospect of treating the total thickness tendency by considering advective changes alone. On the contrary, any large advective change tends to be offset by large values of the other components, so that the total thickness tendency appears as a comparatively small residual.

I am proceeding with the investigation of the dynamic and non-adiabatic components, but the former at any rate is very difficult.

Dr. Bleeker : I am well aware of the systematic way in which Dr. Rossby wants to attack the difficult problems of theoretical meteorology. I have the feeling however, that if Dr. Rossby could find an ideal solution for the barotropic atmosphere or for an idealised baroclinic atmosphere in frictionless motion, that it would be necessary to start from scratch as soon as differential heating was brought into the system of equations.

I am well aware of the difficulties of the theoreticians to solve the problem of thermal circulation and I consider it my task as a synoptician to show how important heating and cooling must be. I want to stress that from my point of view it cannot be concluded that heat and cold sources influence the pressure field only locally as Dr. Rossby suggests. This would certainly be the case on a non-rotating earth but not on a rotating earth. I should like to make my views clearer by considering a situation in which a cross-system leads to cyclone development in the northern hemisphere. Warm air on the east side of the centre is thrown to the north and cools, which as is known from synoptic experience, gives a surface-pressure rise. This again may force cold surface air still further east of the cyclone centre to move southward where it is warmed and pressure falls, *etc.* In such a way a complete breakdown of an originally straight westerly current may result, leading to a system of waves in the westerlies of the higher layers. I have purposely given much attention to the role of heating and cooling. It would be silly, however, to say that we could neglect the purely dynamic factors expressed in the equations of motion. In referring to Dr. Rossby's remarks on the jet stream I should like to say that these are certainly not always present over zones of differential heating. It is striking, however, to see how in mean cross-sections they really are present over zones of surface-isotherm concentration (compare Hess's cross sections, *J. Met.*, **5**, p. 293 with climatological maps of North America). Such comparisons have also convinced me that the jet stream is of thermal origin.

In reply to Dr. Petterssen I should like to say that I am greatly interested in his slides just shown, and look forward to further discussion tomorrow. I think, however, that the zones of cyclogenesis in temperate latitudes on the Atlantic are a result of differential heating since in this area the sea-surface isotherms are crowded.

It is difficult to discuss the remarks of Mr. Matthewman. I have always been of the opinion that analysts have the attitude that their own analyses are best and that a little more objectivity could do no harm to synoptic meteorology. I should like to go through Mr. Matthewman's maps and discuss the analysis, but on a schematic sketch on the blackboard it is impossible to give helpful criticism.

CENTENARY SYMPOSIUM
ON
THE GENERAL CIRCULATION

Held at Oxford on the morning of 30 March 1950

Chairman : E. GOLD, F.R.S.

551.513.1 : 551.515

Some aspects of the general circulation of the atmosphere

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Director of Scientific Services, Air Weather Service, Washington, D.C.

SUMMARY

On the basis of present knowledge of the zonal distribution of incoming and outgoing radiation and of the criteria for dynamic stability of the zonal motion, the conclusion is reached that the radiative processes, in tending to establish radiative equilibrium, create dynamic instability which gives rise to meridional circulations that contribute to the exchange of heat and atmospheric properties.

A generalization of the vorticity theorem is derived and the vorticity budget of the atmosphere discussed. It is shown that the frictional and the nonadiabatic thermal components of the vorticity sources and sinks must balance for each global isentropic sheet, but local balance cannot, in general, be attained. The location of the major vorticity sources and sinks and the transfer of vorticity between them are discussed, and the maintenance of the major circulation system, (such as the Icelandic and Aleutian lows, the subantarctic low-pressure belt, the monsoon systems and the subtropical highs, *etc.*) are explained as a result of this transfer. The existence of local circulation systems associated with mountain ranges, inland water bodies, *etc.*, is accounted for in a similar manner.

The theoretical results are compared with a statistical analysis of the behaviour patterns of the cyclones and anticyclones of the northern hemisphere during the period 1899-1939, and agreement in all essential features is found. Evidence is presented to show that many of the perturbations that develop into travelling cyclones and anticyclones in middle latitudes, originate in the equatorial belt and are brought into the prevailing westerlies with the current around the western extremities of the subtropical anticyclones.

On the basis of the theoretical and statistical results, circulation models for the lower part of the troposphere of the northern hemisphere are proposed.

1. THE PROBLEM : COMPROMISE BETWEEN RADIATIVE AND DYNAMIC EQUILIBRIUM

Through the works of Simpson (1928, 1929) and others, it is known that the system earth-atmosphere receives more radiation than it emits in low latitudes while the reverse is true in high latitudes, the mean temperature being kept constant by ocean and air currents, the latter involving also transfer and liberation of latent heat.

Although the heat and cold sources for the system earth-atmosphere as a whole exhibit a fairly pronounced zonal distribution, very little can be said about the location and intensity of the heat and cold sources if the atmosphere is considered by itself. Thus, for example, the northern part of the North Atlantic constitutes a potent heat source (as far as surface activities are concerned) for all air masses that move down from the arctic while it acts as a relatively weak cold source for air that moves up from the subtropical high. Similarly, the Siberian continent in winter is an effective cold source for air arriving from the Atlantic while it may be almost neutral relative to air moving in from the arctic ice fields.

As pointed out by Sutcliffe (1949), Priestley (1949) and others, the liberation of latent heat is also important, and the processes responsible for this liberation are neither steady nor stationary, although statistically they exhibit a maximum of frequency and intensity in the doldrums, in the subtropical monsoon systems, and in the extratropical cyclone belt.

Loss of heat from the troposphere by radiation is a more or less steady process in all latitudes and at all times, although its intensity varies with the atmospheric composition (cloud, moisture, *etc.*).

To maintain a steady temperature distribution, different from that corresponding to radiative equilibrium, circulation and exchange of mass between the sources and sinks of heat are obviously required. Now, every component of the atmospheric motion, from turbulent eddies and penetrative convection, to cyclones and anticyclones and the larger circulation systems, contribute in various degrees to the required exchange of heat. It appears, therefore, unprofitable to define the general circulation of the atmosphere as that motion which effects the required exchange of heat, for if we do so, we are returned to the general problem of atmospheric motion with all its complex components.

What then is a profitable definition of the general circulation ?

Through the works of Charney (1947), Eady (1949), Berson (1949) and others, it is known that the zonal motion of the atmosphere tends to become dynamically unstable when the Richardson number reaches a certain critical value, and this value is largely determined by the shear along the vertical of the wind. At the critical limit unstable rearrangements of the potential and kinetic energy occur, leading to perturbations of growing amplitudes, and increased meridional exchange. Now, if radiative equilibrium were to be approached, the meridional temperature gradient would increase and so also would the wind shear along the vertical, with a consequent approach to, or transgression of, the limit of stability.

There is an overwhelming amount of synoptic evidence in support of the view that the mid-latitude troposphere is almost always unstable in some part, and that each breakdown of stability leads to meridional circulations with an excess of exchange in other regions. On the other hand, there is no evidence to indicate that the radiative equilibrium has ever been attained. It appears, therefore, that the radiative processes, in trying to establish radiative equilibrium, create dynamic instability and that the critical limit at which instability occurs is such that radiative equilibrium can never be reached. One may, therefore, say that the states of dynamic and radiative equilibrium are mutually exclusive.*

But are we, then, to say that what is commonly called the general circulation is nothing but the statistical manifestation of currents resulting from more or less chaotic release of instability ? If we adopt this point of view the term " general circulation " has no meaning distinct from general atmospheric motion with all its complex features.

It is probably possible to find a workable compromise so that the term " general circulation " may be retained to mean the basic current upon which the disturbances, resulting from dynamic and other instability, develop and along which they are guided without hardly ever disfiguring the basic current to such an extent that it becomes essentially different from the statistical mean motion of the atmosphere. Thus, the problem of the general circulation may be taken to include the maintenance of the basic current and of the patterns shown by the *ensemble* of moving cyclones and anticyclones.

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* In this respect, the atmosphere of the earth may differ essentially from that of other celestial bodies.

In support of this view, and in lieu of a precise definition of the general circulation, the following comments are offered.

It is well known that the statistical mean motion of the atmosphere is characterized by the following systems :

(1) a belt of relatively low pressure at sea level in the equatorial belt with an indistinct pressure distribution aloft ;

(2) belts of high pressure at all levels in subtropical latitudes (say 25-30° N. and S.) ;

(3) a belt of low pressure at sea level around the antarctic ice mass, and semipermanent low-pressure areas at sea level near Iceland and along the Aleutian Chain ;

(4) a vast low-pressure régime at high levels over each of the polar regions ; and

(5) large monsoon systems associated with low-pressure areas over warm subtropical continents.

In addition to these major systems mention might be made of the local circulation systems which develop seasonally over large inland water bodies, such as the Mediterranean, the Black Sea, the Caspian, the Baltic, the Great Lakes of North America, *etc.* Although these minor systems tend to be obscured in the mean pressure charts, they stand out clearly when the frequencies of occurrence of cyclones and anticyclones are considered.

The problem of the general circulation may now, perhaps, be formulated in the following manner : Given the mean temperature distribution of the atmosphere and the approximate location of the major heat and cold source, is it possible, on dynamic and thermodynamic principles, to account for the maintenance of the above-mentioned semipermanent pressure systems and for the tendency of travelling cyclones and anticyclones to follow, in a statistical sense, the observed patterns ?

Before proceeding with the theoretical discussion of this problem, it is convenient to present some statistical information on the normal meridional structure of the atmosphere.

2. THE MEAN MERIDIONAL STRUCTURE

The data contained in the following diagrams have been obtained from the *Normal Weather Maps.** Each cross-section represents the mean for 18 meridional sections at 20-degree intervals starting from the Greenwich meridian.

Figs. 1 and 2 show, for winter and summer respectively, the mean meridional distribution of temperature and potential temperature.** It will be seen that the conditions in the troposphere are almost barotropic in the extreme north and south, with a broad region of baroclinity from sea level to about 10 km between 20° and 70° N. If the difference in humidity between the doldrums and the adjacent dry air of the subtropical cells were considered, an equatorial belt of slight baroclinity would probably become apparent.

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* Published by the U.S. Weather Bureau for the Joint Meteorological Committee. For a discussion on the sources and method of compilation of the data, the reader is referred to that publication.

** The writer is indebted to Dr. H. Wexler for permission to use much of the material presented in Figs. 1-4, which had already been compiled by him.

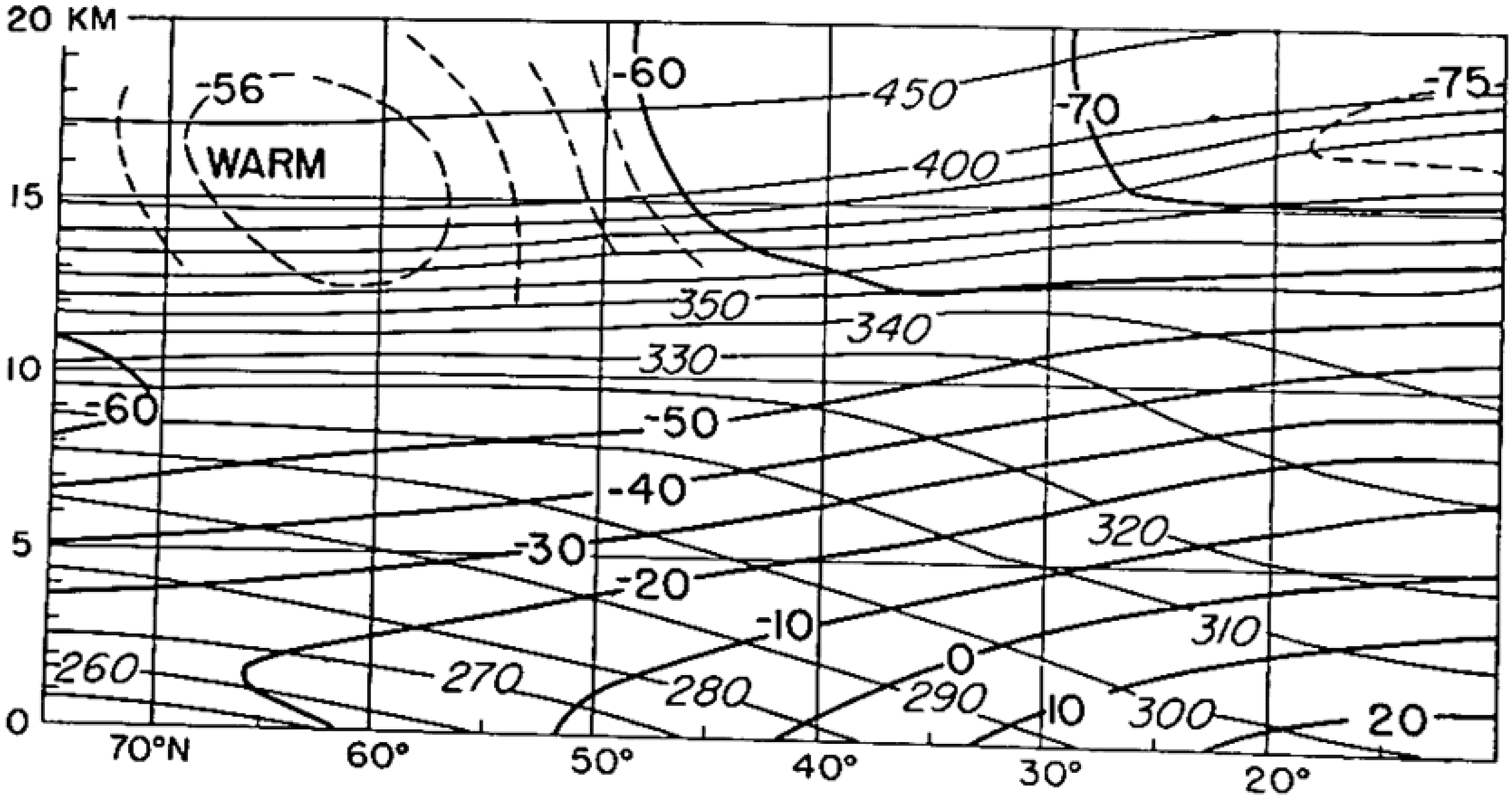

Figure 1. Mean meridional distribution of temperature (°C) and potential temperature (°A) in winter.

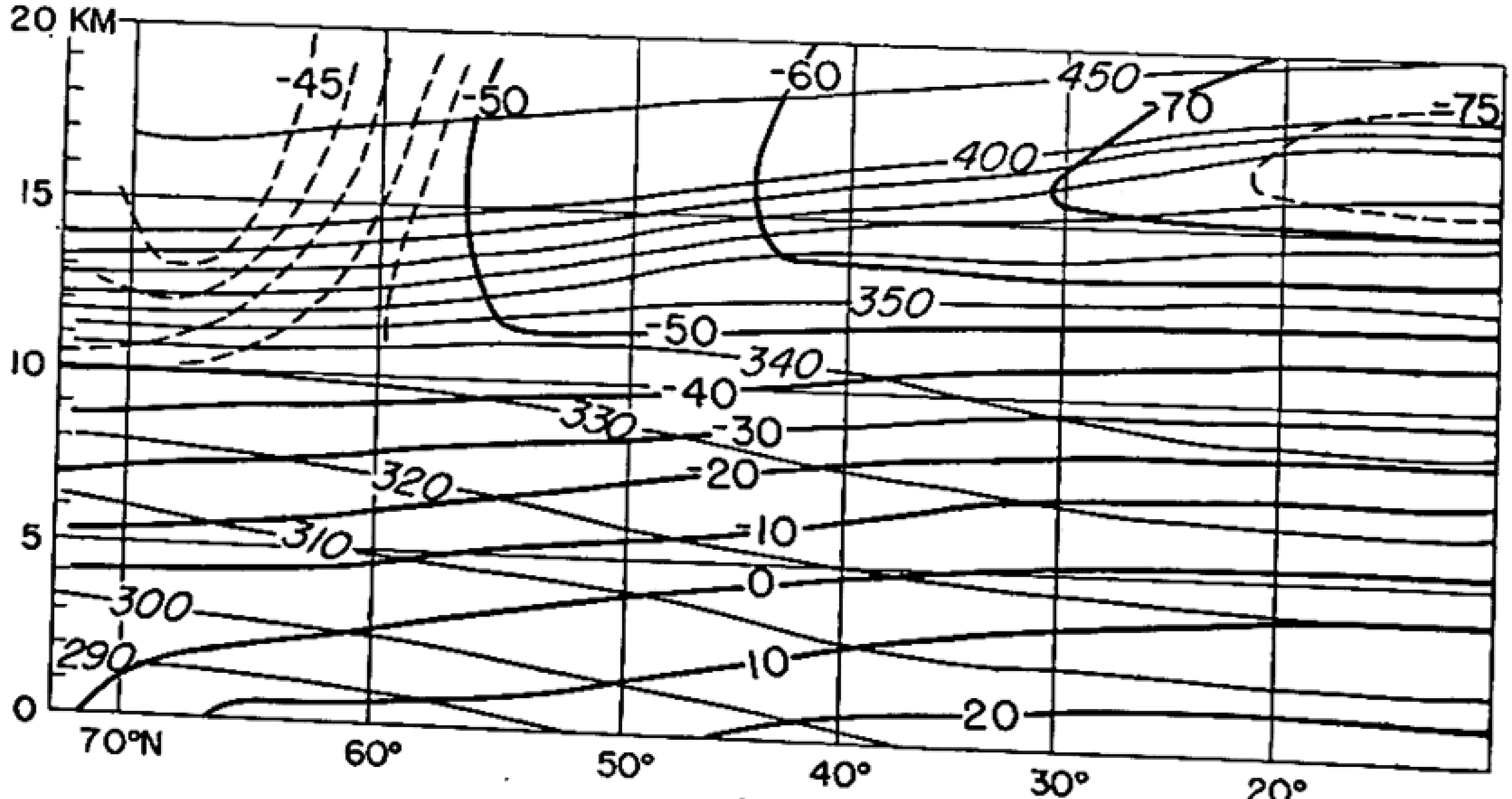

Figure 2. Mean meridional distribution of temperature (°C) and potential temperature (°A) in summer.

It is of interest to note that there is a distinct maximum of temperature in the stratosphere during the winter season at about 60-65°N., which appears to derive from a heat source associated with the sunlit part of the ozone layer. In summer the maximum is found farther to the north, but its poleward extension cannot be determined from the available data.

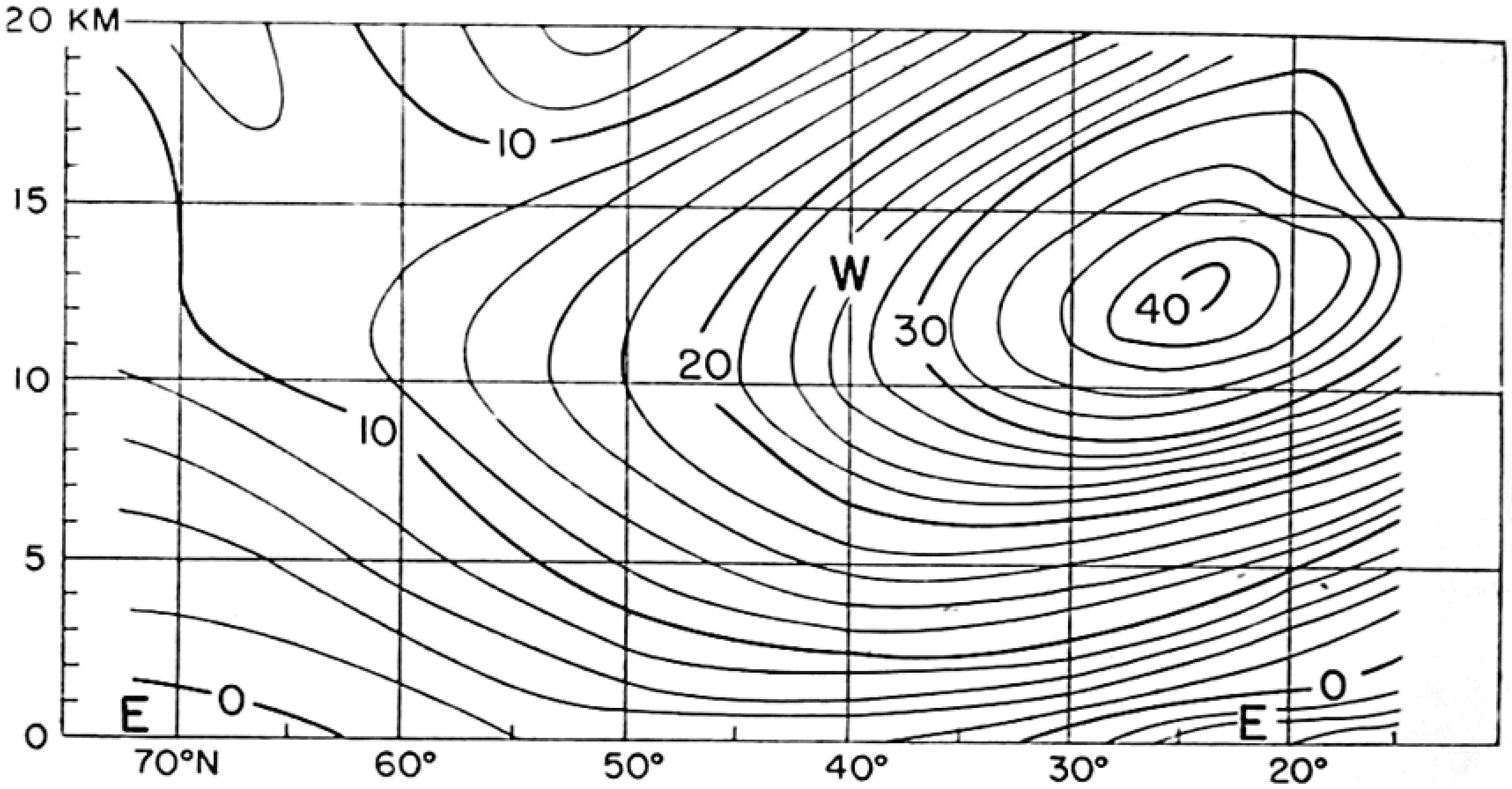

Figure 3. Mean zonal component of the geostrophic wind in winter (m/sec).

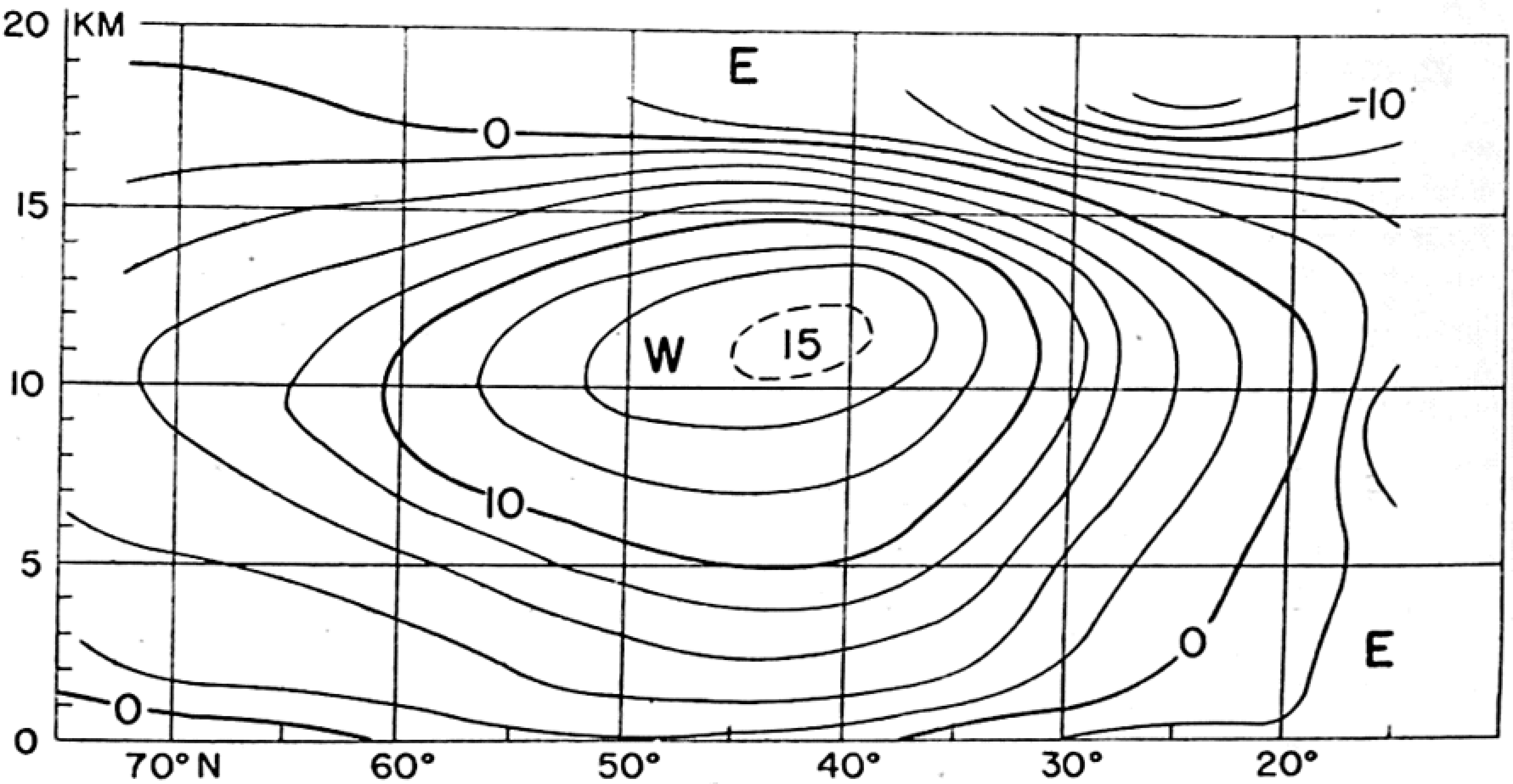

Figure 4. Mean zonal component of the geostrophic wind in summer (m/sec).

Figs. 3 and 4 show the corresponding mean meridional distribution of the zonal component of the geostrophic wind. It will be seen that the maximum west wind occurs about 12-13 km above sea level at about 25°N. in winter, and at about 40-45°N. in summer. The variation along the vertical of the zonal geostrophic wind reflects, of course, the thermal structure shown in Figs. 1 and 2.

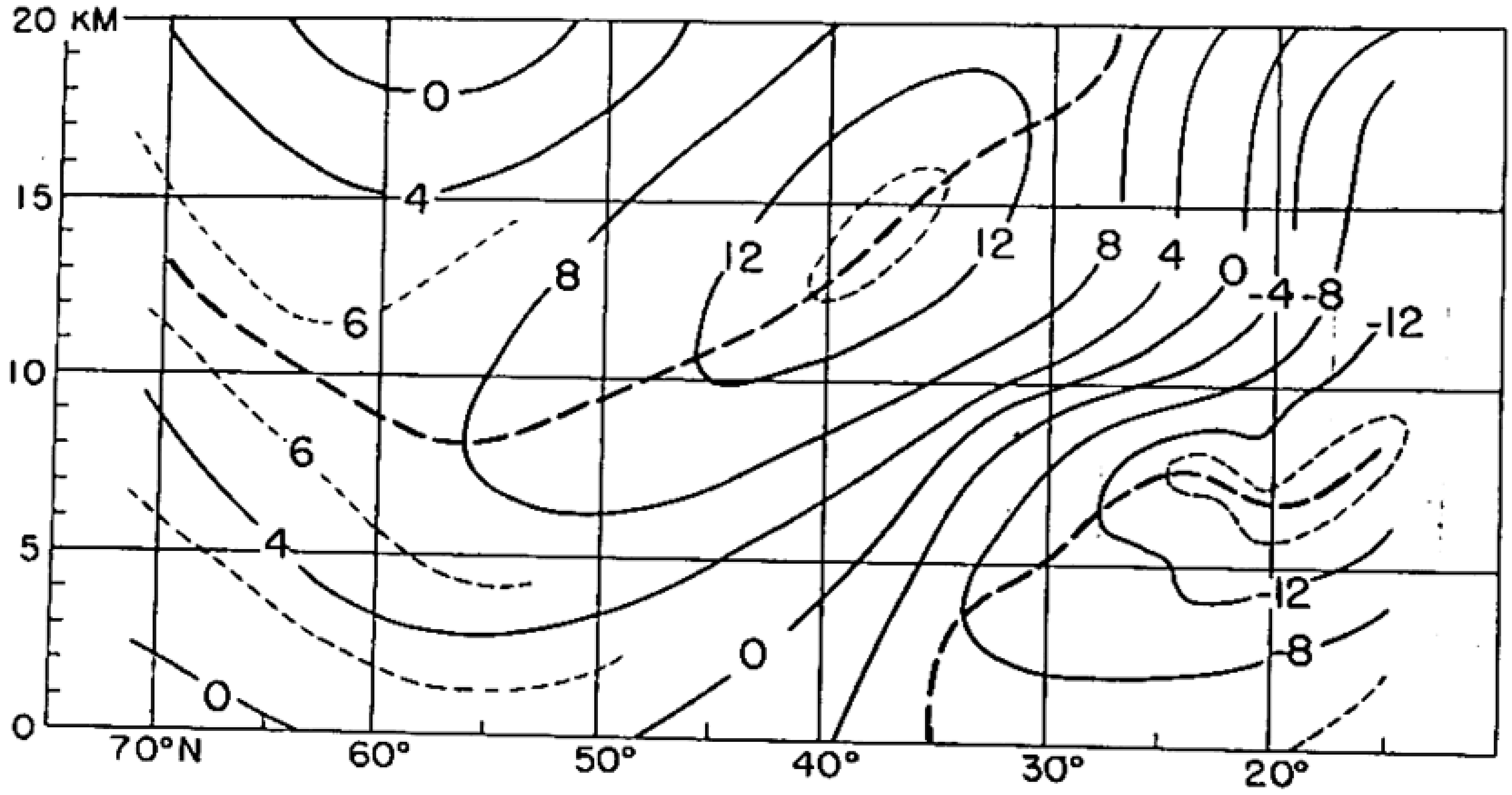

Figure 5. Mean meridional distribution of relative vorticity (10^{-4} sec^{-1}) in winter.

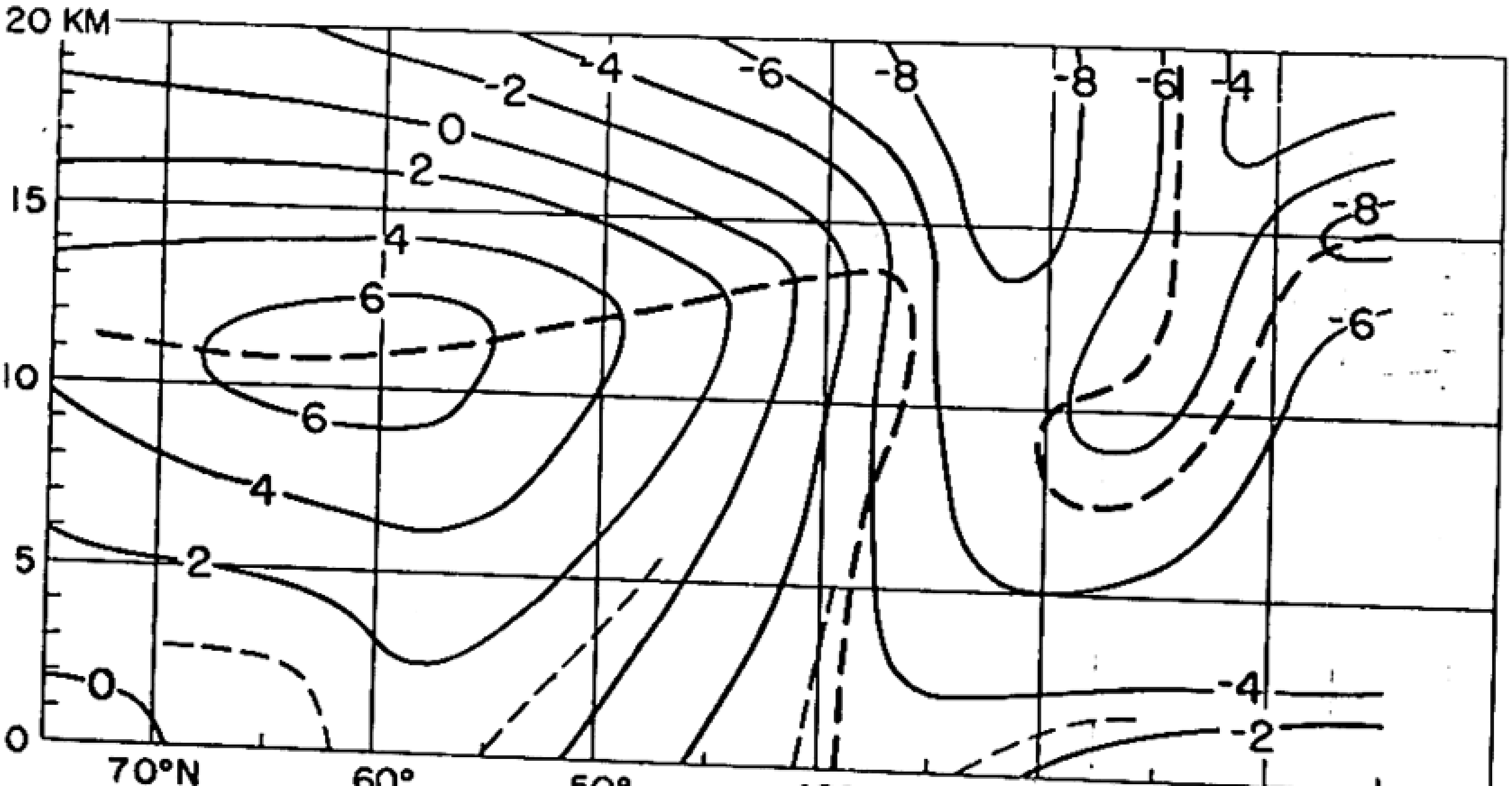

Figure 6. Mean meridional distribution of relative vorticity (10^{-4} sec^{-1}) in summer.

Figs. 5 and 6 show the mean meridional distribution of the component of the relative vorticity normal to the isentropic surfaces* shown in Figs. 1 and 2. The vorticity having been computed from the mean zonal geostrophic wind, its variation along the vertical is just another expression of the mean thermal structure.

It will be seen from Fig. 5 that, during the winter season, a broad band of cyclonic vorticity extends from the polar upper stratosphere and lower troposphere

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* The advantage of considering the vorticity component along the outward normal to isentropic surfaces will be explained in section 5.

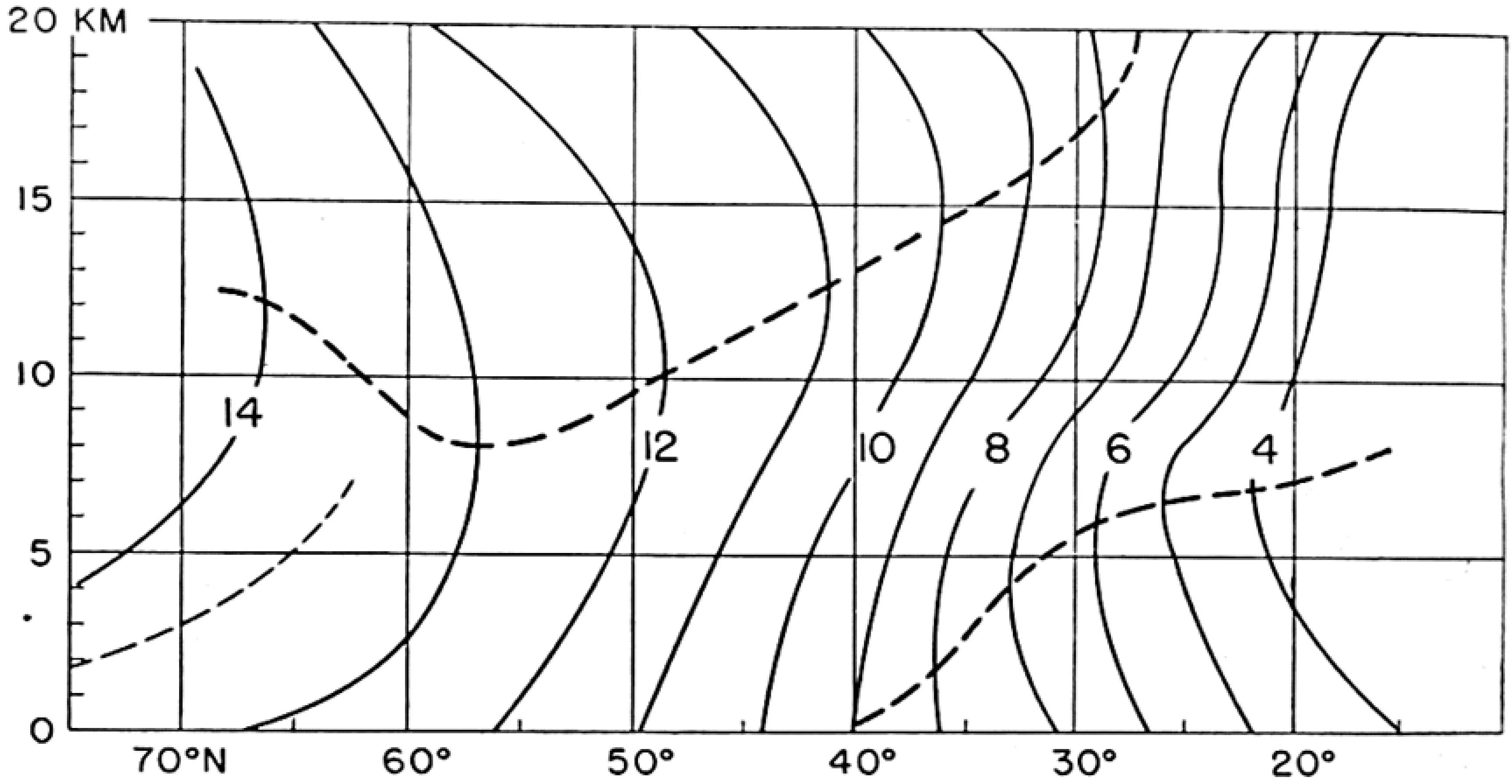

Figure 7. Mean meridional distribution of absolute vorticity (10^{-5} sec^{-1}) in winter.

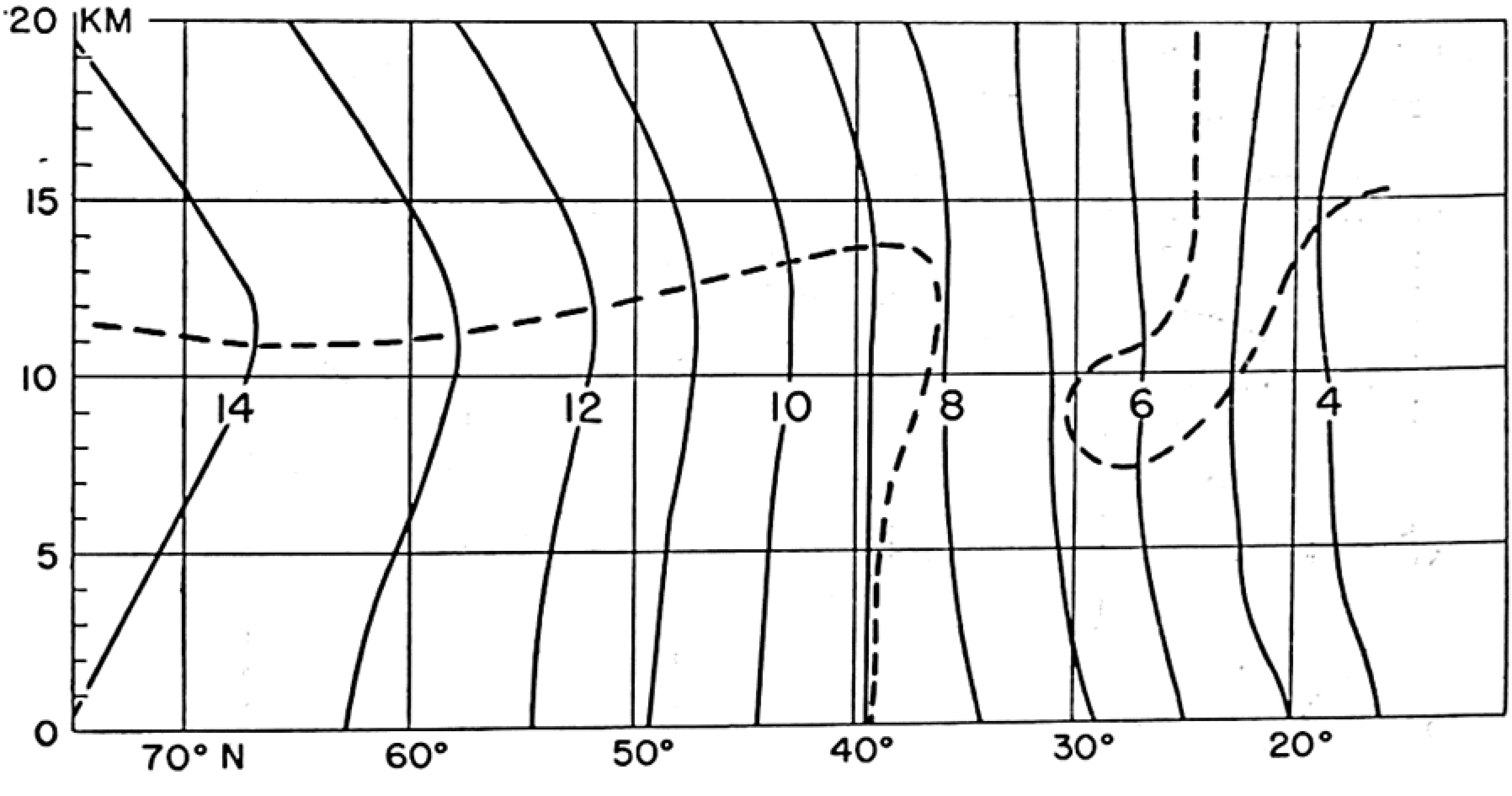

Figure 8. Mean meridional distribution of absolute vorticity (10^{-5} sec^{-1}) in summer.

southward to subtropical latitudes, being bounded to the south by a zone of rapid transition to anticyclonic vorticity. In summer (Fig. 6) conditions are similar, except that the anticyclonic régime extends farther to the north, particularly in the stratosphere. Figs. 7 and 8 show similarly the distribution of the absolute vorticity.

In later sections, we shall have occasion to refer to these diagrams which to a certain extent may be taken to represent the essential features of the mean circulation, or the basic current upon which the perturbations are superimposed.

3. THE VORTICITY EQUATION

Let δC_r denote the circulation of the relative velocity around the border of the area element δA, and let q be the component of the relative vorticity along the outward normal to the area δA. Then, according to Stokes's theorem

$$C_r = \int_A q\delta A = A\bar{q} \qquad (1)$$

where $\bar{q}$ is the mean value of q.

Now, the component of the earth's vorticity along the outward normal of the area δA is obviously $2\,\Omega\cos\psi$, where ψ is the angle from the polar axis to the said normal. The corresponding circulation, due to the earth's rotation, is therefore

$$\delta C_c = 2\Omega\cos\psi\,\delta A$$

or, for a finite area

$$C_c = 2\Omega A\overline{\cos\psi}$$

where $A\overline{\cos\psi}$ is the projection on to the equatorial plane of the area A.

The absolute circulation C around the border of the finite area A is therefore

$$C = A(\bar{q} + 2\Omega\overline{\cos\psi}) = A\bar{Q} \qquad (2)$$

where $\bar{Q}$ is the mean component of the absolute vorticity normal to the area A.

Now, according to Bjerknes's circulation theorem

$$\frac{dC}{dt} = N + C_F \qquad (3)$$

where N is the number of isosteric-isobaric solenoids contained in the area A, and C_F is the circulation of the frictional force around the border of A.

Substituting (2) into (3), we obtain

$$\frac{d\bar{Q}}{dt} = \frac{N}{A} + \frac{C_F}{A} - \frac{1}{A}\frac{dA}{dt}\bar{Q} \qquad (4)$$

Since (2) holds for infinitesimal as well as for finite areas, the bar symbol may be omitted. Furthermore, introducing

$S = N/A =$ the mean number of solenoids per unit area

$F = C_F/A =$ the vorticity of the frictional force,

and noting that according to Gauss's theorem

$\dfrac{1}{A}\dfrac{dA}{dt} = \mathrm{div}_A\mathbf{V} =$ the mean two-dimensional divergence over the area A,

Eq. (4) may be written

$$\frac{dQ}{dt} = S + F - \frac{1}{A}\frac{dA}{dt}Q = S + F - Q\,\mathrm{div}_A\mathbf{V} \qquad (5)$$

In this form the vorticity theorem may be applied stringently to infinitesimal as well as to finite areas of any orientation in space, a circumstance that renders it particularly suitable for discussions of the large-scale currents of the atmosphere.

Expanding the left of (5) and rearranging the terms, the vorticity theorem may be written in either of the following forms

$$\frac{\partial Q}{\partial t}+\nabla_A\cdot(QV)=S+F-V_n\frac{\partial Q}{\partial n} \quad . \quad . \quad . \quad (6)$$

or

$$\frac{\partial Q}{\partial t}+\nabla\cdot(QV)=S+F+\frac{\partial V_n}{\partial n}Q \quad . \quad . \quad . \quad (7)$$

where n measures length along the outward normal to the surface considered, and V_n is the component of the velocity along that normal.

The term $\nabla_A\cdot(QV)$ denotes the export of vorticity across the boundary of the area A, and $\nabla\cdot(QV)$ (which is the three-dimensional divergence of the vector QV), denotes the export of vorticity through the boundary of a volume.

It may be noted in passing that (7) is similar to the equation of continuity of mass, except that the depletion (*i.e.*, $-\partial Q/\partial t$) is not equal to the export [*i.e.*, $\nabla\cdot(QV)$], the solenoidal field, the vorticity of the frictional force, and the normal stretching (*i.e.*, $\partial V_n/\partial n$) representing the sources of vorticity. Similarly, according to (6), the vorticity sources in an arbitrary surface are due to solenoids, friction, and normal advection.

4. SHORT-TERM VARIATIONS IN VORTICITY

Although friction and supply of heat are the controlling factors in the maintenance of the general circulation, both of these factors may usually be disregarded if we consider the motion of the free atmosphere over short intervals of time. To gain some insight into the mechanism of the vorticity transfer, we shall here consider adiabatic and frictionless motion. Although these considerations will have some bearing upon the short-term behaviour of the perturbations, they can throw no light upon the problem of the maintenance of the general circulation.

For convenience, we choose an area A in an isentropic surface (*i.e.*, $S = 0$). Since the motion is assumed to be adiabatic and frictionless, S and F vanish identically, and Eq. (5) reduces to

$$\frac{dQ}{dt}=-\frac{1}{A}\frac{dA}{dt}Q \quad . \quad . \quad . \quad . \quad (8)$$

or, upon integration

$$A\,Q = A_0\,Q_0$$

where subscript zero denotes the initial value.

Since A and A_0 are positive by definition, it will be seen that the sign of the absolute vorticity cannot change as long as the motion is adiabatic and frictionless.

Observations show that the absolute vorticity of the large-scale motion is always positive, suggesting that friction and nonadiabatic temperature changes are generally of less importance than the divergence term. In rare cases indications of negative absolute vorticity have been found as local and temporary occurrences. Since we are here concerned to discuss the large-scale currents of the atmosphere, it will be assumed that the absolute vorticity is positive.

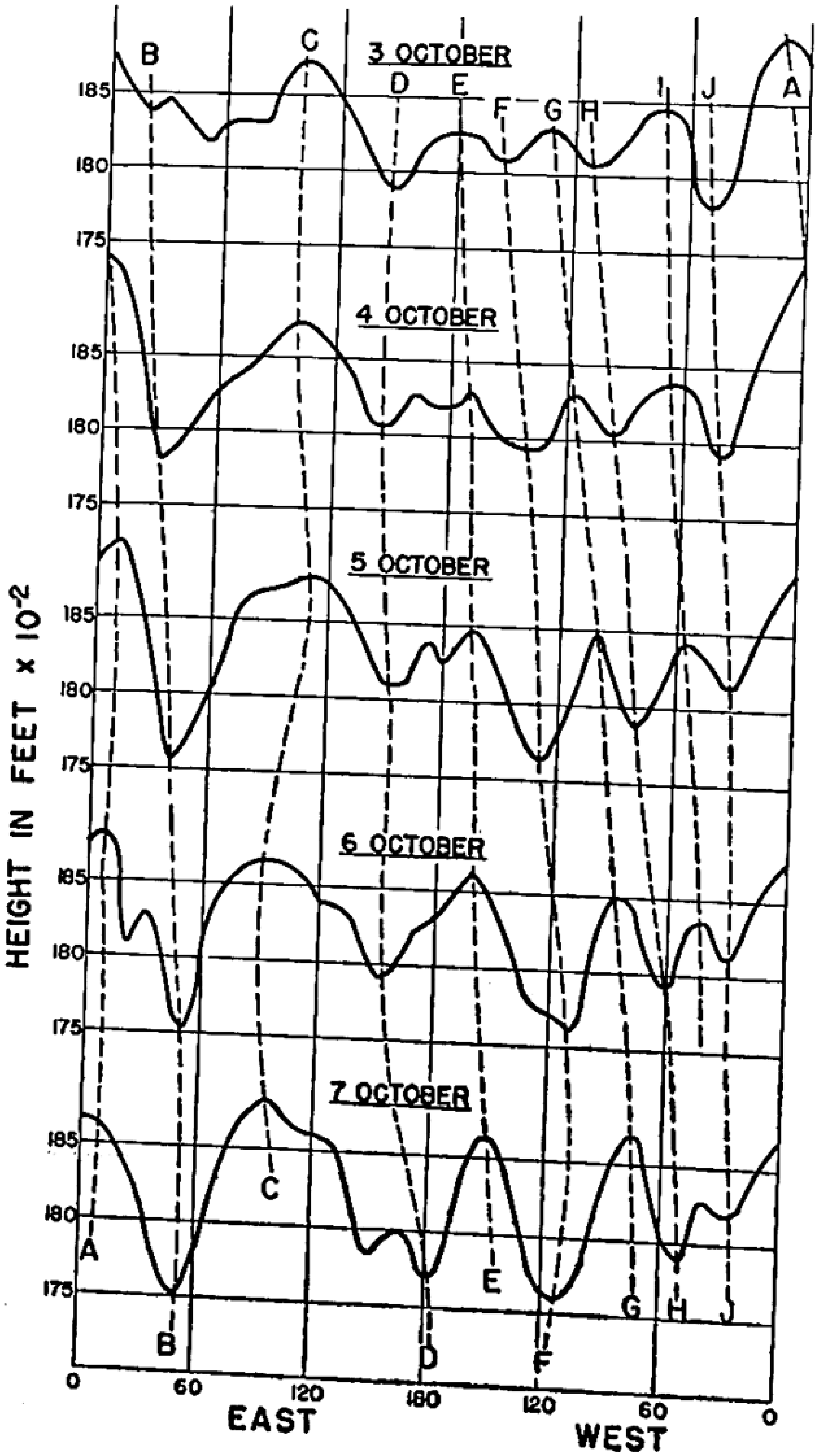

Figure 9. Zonal profile of mean height of the 500-mb surface between 45 and 60°N.

We shall now consider an area A so large that it contains several systems of convergence and divergence. Since dA/dt is the export across the boundary of A, it is evident that the term on the right decreases and tends to zero for very large areas. It follows then that the *mean* (but not the local) absolute vorticity tends to be conserved. From this, again, it follows that if somewhere within a large area A there occurs a localized development leading to, say, increased absolute vorticity, there must, elsewhere in the area, exist an opposite development so as to maintain a constant (or almost constant) total.

Examples of these *sympathetic developments* are seen almost daily on the synoptic upper air charts (where the assumptions of adiabatic and frictionless conditions are reasonable), but the point that it is desired to emphasize here is that these developments do not necessarily affect the vorticity balance of the general circulation to any large extent.

Since a more detailed discussion of these synoptic developments will be given elsewhere, it suffices here to refer to Fig. 9 which shows successive longitudinal profiles of the 500-mb surface between 45° and 60°N. during a five-day period characterized by excessive developments in the pressure field.

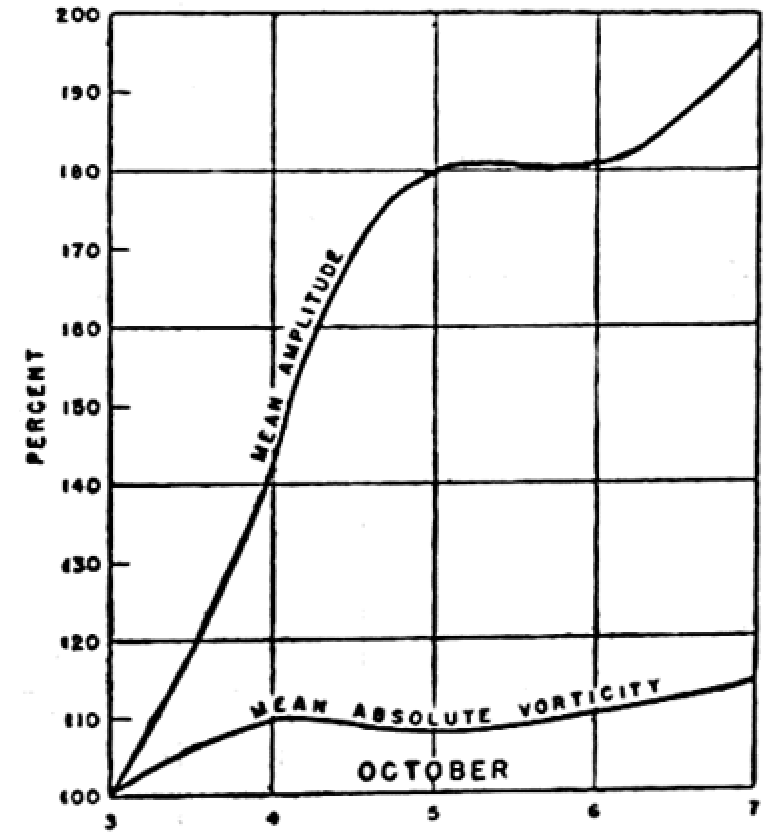

Figure 10. Percentage increase of the mean amplitude of the several pressure waves and of the absolute vorticity corresponding to Fig. 9.

On 3 October 1949 two localized developments in the form of cyclone waves at sea level are visible as the minor troughs B and F. As these developed, the mean amplitude of the several pressure waves increased by about 100 per cent (Fig. 10) while the mean absolute vorticity of the belt increased only by about 15 per cent.

The development described above was characterized by a change from a fairly strong zonal to a pronounced meridional circulation with large amplitudes, and these fluctuations are, apparently, associated with breakdowns of the stability of the zonal current which we shall not be concerned to discuss here. It is of interest, however, to note that during the periods of predominantly zonal motion, there is a deficit of exchange of mass (and heat) between low and high latitudes, and during the intervening periods there is an excess of such exchange. It appears, therefore, that the exchange of mass between high and low latitudes tends to be realized through unstable outburst rather than through a steady flow symmetrical around the pole.

Whether or not these individual outbursts of cold air from, and invasions of warm air into, the polar region should be regarded as components of the general circulation is, and will probably remain, a moot question. However, even in the most perturbed situations, the circumpolar vortex in the middle and upper troposphere and lower stratosphere is still overwhelming, in the sense that the pressure is, level for level, low over the polar region and high in the subtropical belt. The problem of accounting for the maintenance of the circumpolar vortex appears, therefore, to be a legitimate one.

5. THE VORTICITY BALANCE

We shall now return to the problem of the general circulation and endeavour to explore the relation between the vorticity sources and the sources of heat and cold. From a formal point of view,[*] the effect of these latter sources may be expressed through the variation of potential temperature (θ) experienced by a moving unit of air, *viz.*,

$$\frac{d\theta}{dt}=\frac{\partial\theta}{\partial t}+\mathbf{V}\,.\,\nabla_A\,\theta+V_n\,\frac{\partial\theta}{\partial n}\quad .\quad .\quad .\quad .\quad (9)$$

Here, the second term on the right denotes the advection in the surface A, and the third term the advection through that surface.

The general circulation may now be assumed to be characterized by

$$\frac{1}{T}\int_0^T\frac{\partial Q}{\partial t}\,\delta t=0\quad ;\quad \frac{1}{T}\int_0^T\frac{\partial\theta}{\partial t}\,\delta t=0$$

where T is a sufficiently long span of time (t).

Applying this to (6) and (9), and writing the variables as a mean value plus a deviation, it will be seen that the corresponding equations for the mean state will contain terms composed of mean values only, supplemented with certain terms which depend essentially upon the correlations between **V** and the spatial variations of θ and Q. Since these correlations are extremely difficult to determine, we shall assume that the terms containing them are small in comparison with the terms composed of mean values only. Since this assumption cannot be justified by reference to dynamic and thermodynamic principles, its validity must be determined by comparison of derived results with observation.

The above assumption is equivalent to considering the mean state as an individual stationary[**] state (*i.e.*, $\partial\theta/\partial t=0\ ;\ \partial Q/\partial t=0$).

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[*] In specific cases, this formal introduction of the effect of heat and cold sources must be supplemented by thermodynamic considerations, particularly in regard to the relative position of the sources of supply and removal of heat. (See section 9)

[**] Evidence will be presented in later sections to show that this assumption is satisfactory in the polar region and in the intertropical belt; while in the belt of prevailing westerlies, which is dominated by travelling cyclones and anticyclones, the non-stationariness of Q, θ and V must be considered.

It is convenient to choose to represent the conditions in an isentropic surface, rather than on equigeopotential or isobaric surfaces. Since

$$\mathbf{V} \cdot \nabla_A \theta = 0 \text{ and } S = 0$$

in an isentropic surface, equations (6) and (9) may be combined to give

$$\nabla_A \cdot (Q\mathbf{V}) = F - \frac{\partial Q/\partial n}{\partial \theta/\partial n}\frac{d\theta}{dt} = F - \frac{\partial Q}{\partial \theta}\frac{d\theta}{dt} \qquad . \qquad . \qquad (10)$$

which specifies the *combined dynamic and thermodynamic control of any stationary circulation*.

Now, the term on the left is a measure of the export of vorticity along the isentropic sheets, or the *intensity of the vorticity source*. Eq. (10) then states that the source intensity is equal to the vorticity of the frictional force, supplemented by a term which depends upon the rate of heating or cooling, or the local intensity of the heat or cold source through which the air is moving. We may, therefore, speak about the *frictional* and the *thermal* components of the source of vorticity.

The sign of the frictional component in the lower part of the troposphere is readily determined. If the frictional force is to act mainly against the motion, the sign of the vorticity of the frictional force must be opposite to that of the vorticity of the relative motion. Hence, cyclonic systems constitute frictional sinks, and anticyclonic systems constitute frictional sources of vorticity.

The sign of the thermal component varies with the spatial distribution of the vorticity and with the degree of static stability.* Since we are here concerned to discuss a state which in all essentials resembles the mean state of the atmosphere, we may take $\partial\theta/\partial n$ to be positive everywhere. It will then be seen from (10) that the thermal component of the intensity of the vorticity source

(1) is *positive* where the air is *cooled* in a field where the vorticity *increases*, or *heated* where the vorticity *decreases* in the direction of increasing entropy ; and

(2) is *negative* where the air is *heated* in a field where the vorticity *increases*, or *cooled* where the vorticity *decreases* in the direction of increasing entropy.

It will be shown in the following sections that the atmosphere may be divided into large regions within each of which the thermal component is not balanced by the frictional component, with the result that vorticity is exported from certain sources and deposited in certain sinks. It is, however, evident that balance must be established within each isentropic sheet. Integrating (10) over an isentropic surface covering the whole globe, we obtain

$$\int_A F \delta A = \int_A \frac{\partial Q/\partial n}{\partial \theta/\partial n}\frac{d\theta}{dt}\delta A \qquad . \qquad . \qquad . \qquad . \qquad (11)$$

where the integral is to be taken over a closed isentropic surface. Thus, for each global isentropic sheet, the frictional and the thermal components must balance if

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* On account of the slight slope of the isentropic surfaces, $\partial\theta/\partial n$ may be identified with the vertical ascendent of potential temperature. Similarly, if q is the component of the relative vorticity normal to the isentropic surface,

$$Q = q + 2\,\Omega \cos \psi$$

Since the slope of the θ-surface is very small (*i.e.*, 10^{-3}), we may, for all practical purposes put $\cos \psi = \sin \phi$, where ϕ is the angle of latitude.

the motion is to be stationary. Neglecting the transequatorial export of vorticity (such as may be associated with the annual variation), Eq. (11) may be taken to hold for each hemispherical isentropic sheet.

6. THE POLAR REGION

We shall consider a circular continent (or ice field) centred at the pole and surrounded by water of appreciably higher temperature, such as may occur during the winter season. The isentropic surfaces would then form domes over the cold continent, as shown in Fig. 11, with a maximum of concentration of isotherms along the coast.

Now, any air mass that invades the arctic in winter will be cooled (*i.e.*, $d\theta/dt < 0$), partly through loss of heat to the underlying surface and partly through radiative loss to space. The polar region being surrounded horizontally by warmer air, the horizontal poleward pressure force must increase with elevation, and if the pressure force is to be balanced mainly by the Coriolis force, the relative (and absolute) vorticity must increase with elevation. Since $\partial\theta/\partial n > 0$, it will be seen from (10) that the thermal component of the vorticity source must be positive.

As to the frictional component it may be noted that the pressure distribution near the earth's surface is frequently anticyclonic (see Fig. 13), and so is the relative vorticity of the motion. The vorticity of the frictional force in the lower troposphere is, therefore, cyclonic, with the result that both the frictional and the thermal components of the vorticity source are positive. Since stagnant conditions are typical of the central arctic, the frictional component is likely to be of less significance than the thermal one.

From the above and from Eq. (10), it will be seen that *absolute* vorticity must be exported from the polar cap, along the isentropic sheets, down to sea level, broadly as indicated in Fig. 11. Now, since this export is in the direction of decreasing Coriolis parameter, it follows that there must be an even larger export of relative

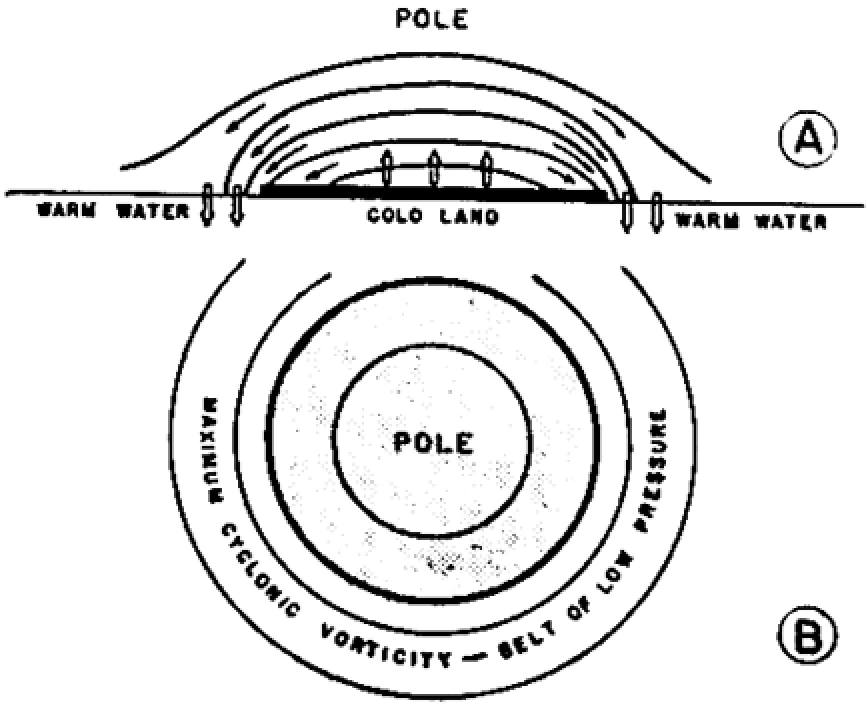

Figure 11. A: Export of vorticity along the isentropic sheets from a polar continent; double-shafted arrows indicate transfer of vorticity to and from the earth's surface.
B: The resulting pressure field at sea level.

cyclonic vorticity down to sea level in the belt of maximum concentration of isotherms. If the pressure force is to constitute the main balance of the Coriolis force in this belt, a depression of the isobaric surfaces, or a belt of low pressure, must be maintained in the region of maximum concentration of isotherms. Again, if the frictional force near sea level in this belt is to act mainly against the wind, the vorticity of the frictional force must be anticyclonic, with the result that vorticity is imparted to the earth's surface. It will thus be seen that the polar region constitutes a source, and the subpolar low-pressure belt a sink of relative cyclonic vorticity.

The circulation described above is typical of the antarctic and subantarctic where the distribution of land and water is fairly symmetrical around the pole.

In the north-polar region the conditions are far more complex. Here the northern parts of the North-American and Eurasian continents act as effective cold sources during the winter season. The isentropic surfaces slope from the interior of these continents, as well as from the polar cap, downward to the coasts, with the result that cyclonic vorticity is exported zonally as well as meridionally towards the northern parts of the North Atlantic and North Pacific Oceans, which areas constitute the principal frictional sinks in high latitudes during the cold season. Thus, while the export of vorticity from the antarctic cold source contributes to the maintenance of a circumpolar belt of low pressure in the subantarctic, the corresponding export from the arctic and the adjacent continental cold sources, contributes to the

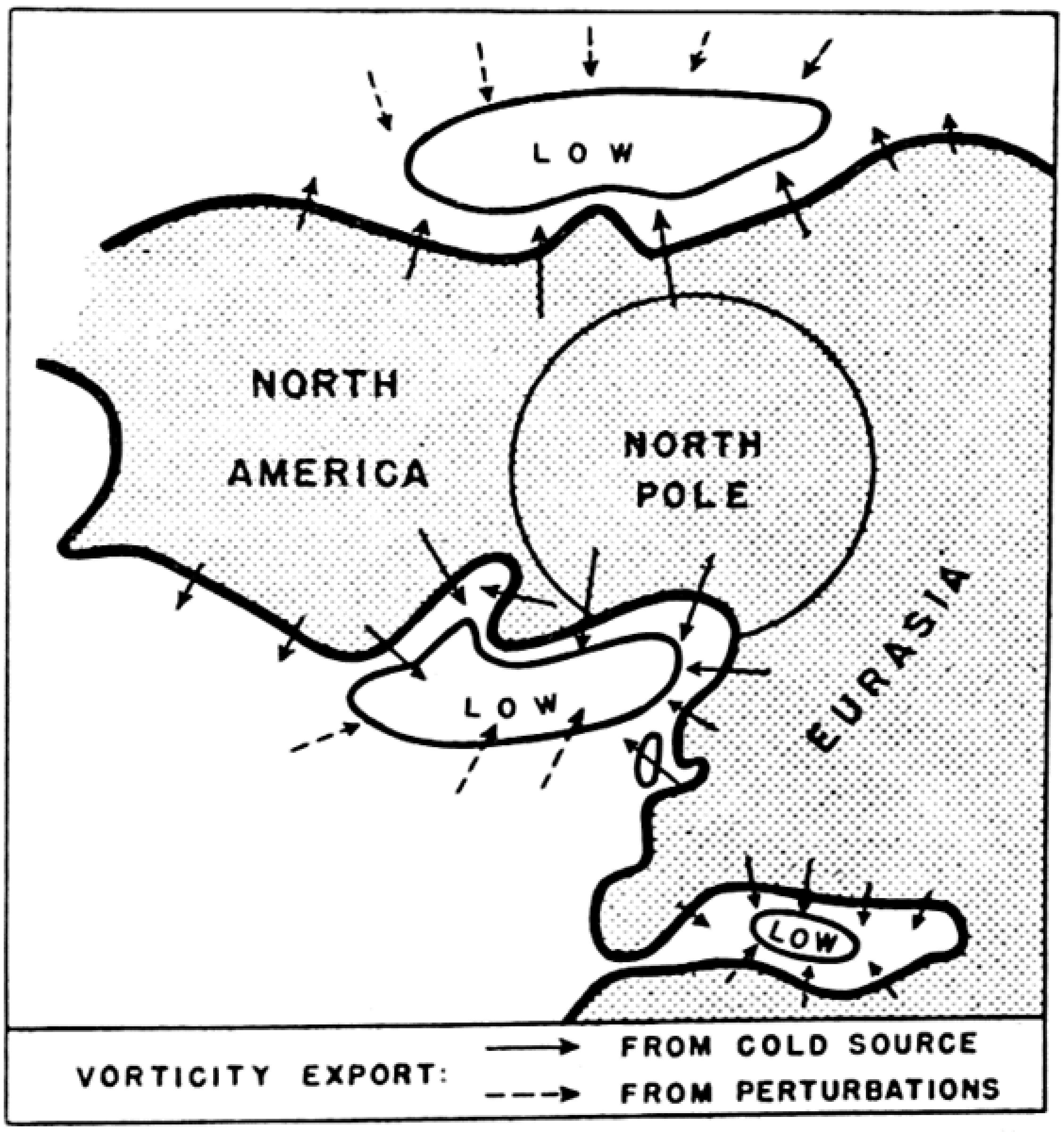

Figure 12. Diagrammatic representation of the flux of vorticity from the arctic icefields and cold continents.

maintenance of the semipermanent low-pressure systems known as the Icelandic and the Aleutian lows. In addition a third semipermanent low must exist in the Mediterranean area during the winter season, as a result of export of vorticity from the European source. This export is particularly effective since it is in the direction of rapidly decreasing Coriolis parameter.

In the same manner as described above, the isentropic surfaces would slope from land downward to the shores of bays and inland water bodies surrounded by cold land, and cyclonic vorticity would be exported along the isentropic sheets down to these water surfaces, which, in turn, constitute frictional sinks of vorticity.

Fig. 12 shows diagrammatically the conditions in high and middle latitudes during the winter season. In summer, when the northern continents are warm, the main temperature contrast is found, not along the border between ice and water, but along the northern coasts. The main export of vorticity in the northern regions is then from sea to land, or from cold to warm ocean currents.

Statistical evidence will be presented in later sections to show that the amounts of vorticity transported in the manner described above is appreciable.

7. THE MID-LATITUDE BELT

The derivation leading from Eqs. (6) and (9) to (10) was based upon the assumptions of stationariness of Q and θ, and noncorrelation between the variations of certain variables. Both of these assumptions appear to be weak as far as the conditions in the mid-latitude belt are concerned.

In the first place, this belt is dominated by large-scale unstable developments resulting in an almost continuous succession of travelling cyclones and anticyclones, with rapid variations in the vorticity field.

In the second place, there appear to exist appreciable correlations between the motion and the supply of heat. This is true not only of the heat received from, or given off to, the earth's surface, but also of the heat supplied through condensation processes. Although in the mean, the relative (and the absolute) vorticity in the mid-latitude belt increases with elevation, the liberation of latent heat associated with fronts occurs most frequently when the vorticity decreases with elevation. The heat liberated through condensation processes is, therefore, a sporadic source that, on the whole, is active when the vorticity distribution deviates appreciably from the mean state. In view of this, it appears unreasonable to apply Eq. (10) to the mid-latitude belt.

In order to gain some insight into the mechanism of the vorticity transfer associated with the travelling cyclones and anticyclones, we shall return to the complete vorticity equation and apply it to a level surface. Putting

$$Q = q + 2\,\Omega \sin\psi = q + f$$

where f is the Coriolis parameter, Eq. (5) may be written

$$\frac{\partial q}{\partial t} + \mathbf{V}\,.\,\nabla q + \mathbf{V}\,.\,\nabla f = S + F - \mathrm{div}_H \mathbf{V}\,.\,(q+f)$$

where $\mathrm{div}_H \mathbf{V}$ is the horizontal divergence.

Applying this equation to sea level, where the vertical component of the velocity vanishes, we obtain for the horizontal ascendent of the "vorticity tendency" through a cyclonic or anticyclonic centre in which the relative vorticity is a maximum or a minimum

$$\nabla \frac{\partial q}{\partial t} = \nabla S - \mathrm{div}_H \mathbf{V} \,.\, \nabla f - (q+f)\, \nabla(\mathrm{div}_H \mathbf{V}) \qquad . \qquad . \qquad (12)$$

It is assumed here that the vorticity of the frictional force has a maximum, or minimum, in the centre (*i.e.*, $\nabla_H F = 0$).

Now, a cyclonic centre, being a centre of maximum relative vorticity, must move in the direction of the ascendent of the vorticity tendency, while an anticyclonic centre must move in the opposite direction.

The last term on the right represents a component of motion from divergence to convergence for cyclonic centres, and from convergence to divergence for anticyclonic centres. By examination of the remaining terms, it will be seen that it is this term that accounts for the normal zonal motion of the travelling pressure centres.*

The second term on the right, which depends upon the gradient of the Coriolis parameter, represents a meridional motion of the pressure centres, such that:

(1) a cyclonic centre moves northward, or southward, according as there is convergence, or divergence, in the centre ; and

(2) an anticyclonic centre moves northward, or southward, according as there is divergence, or convergence, in the centre.

Now, in the lower troposphere convergence towards cyclonic centres and divergence out of anticyclonic centres are the normal occurrences, with the result that the variation in the Coriolis parameter would cause both cyclonic and anticyclonic centres to move with a poleward component. Above the level of non-divergence (usually above, say, 600 mb) the distribution of divergence is reversed, with the result that both cyclonic and anticyclonic centres would move with an equatorward component.** In the upper atmosphere, however, this tendency may be somewhat modified by the distribution of the vertical velocity. The modifying influence of the vertical velocity would, however, vanish in vortices with vertical axes.

Considering next the first term on the right of (12), it will be seen that cyclonic centres tend to move toward the side where the concentration of isosteric-isobaric solenoids is a maximum, whereas anticyclones tend to move toward the side where there is a minimum of such concentration. Now, the largest solenoidal concentration is normally found in subpolar latitudes, while the most uniform air is found in subtropical and lower latitudes. Hence, on the average, both the first and the second terms on the right of Eq. (12) tend to drive the cyclone centres northward, while in the case of anticyclones, the two terms counteract one another. Simple computations show that the order of magnitude of the solenoidal term is the same as that of the gyroscopic term in low and middle latitudes. Now, since the solenoidal term increases northward while the gyroscopic term increases southward, there will be a tendency for subpolar anticyclones to slide southward and for low-latitude anticyclones to slide northward, and for the ensemble of anticyclones to find equilibrium somewhere in middle latitudes.

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* It is assumed here that the absolute vorticity is positive. (See section 4).

** It is of interest to note that a series of high-level cyclones and anticyclones in middle latitudes would, if seen from the sun, behave in the same manner as the sunspots seen from the earth.

On the same principles, cyclone centres would move northward, penetrate the zone of maximum concentration of solenoids, and find equilibrium in some high latitude where the solenoidal and gyroscopic terms balance (*i.e.*, with maximum of concentration of solenoids to the south of the centre).

Finally, it may be noted that $\nabla f = 0$ at the poles, and since the solenoidal concentration normally vanishes there, both cyclones and anticyclones can find equilibrium when centred at the pole. Thus, the low-level anticyclones and the high-level cyclones that are often found over the north pole are in a stable position.

From the above it follows that, on account of the meridional thermal structure and the variation in the Coriolis parameter, a separation of the travelling vorticity systems takes place in middle latitudes, such that the cyclonic systems move northward toward the fringe of the arctic, while anticyclonic systems find equilibrium in lower latitudes. Accordingly, the travelling disturbances, resulting from dynamic instability in middle latitudes, contribute to the maintenance of the subpolar low-pressure systems and to the subtropical high-pressure belt.*

As was shown in the foregoing section, the subpolar low-pressure systems are also maintained through export of cyclonic vorticity from the polar cold source. A factor contributing to the maintenance of the subtropical anticyclones may be sought in the radiative cooling of the cloudless air aloft, as suggested by Sutcliffe (1949) on the basis of an investigation by Robinson. Inasmuch as the cloudlessness is ascribed to anticyclonic subsidence, the radiative cooling cannot be regarded as the primary cause of these anticyclones, although it may add to their stability.

8. The intertropical belt

It is convenient first to consider the trade-wind belt which is characterized by a remarkably high degree of stationariness. Here, Eq. (10) may be applied with confidence.

On the equatorial side of the axis of the subtropical high-pressure belt the vorticity of the motion near the earth's surface is anticyclonic, and if the frictional force is to act against the motion, the vorticity of the frictional force must be cyclonic and render a positive contribution to the vorticity source.

Since the air motion near the earth's surface is equatorward, the air is heated from below, consequently $d\theta/dt > 0$. Furthermore, since the intertropical belt is surrounded horizontally by colder air, the horizontal poleward pressure force must increase with elevation. Now, if the motion at high levels over the trade-wind belt is to be predominantly of the geostrophic type, the relative (and absolute) vorticity must decrease with elevation. It will then be seen from Eq. (10) that the frictional and the thermal component of the vorticity source are both positive in the lower part of the trade-wind belt, and hence, absolute vorticity must be exported from the trade-wind belt.

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* Some of the foregoing results are in agreement with the findings of Rossby (1948, 1949).

Having thus identified a subtropical source of vorticity, the area of which covers a large part of the earth's surface, it is necessary to find adequate sinks in which this vorticity can be deposited. At least three such sinks are immediately apparent.

(1) Part of the absolute vorticity is obviously exported to the doldrums. Since this export is in the direction of rapidly decreasing Coriolis parameter, an even larger amount of relative cyclonic vorticity must be supplied to the doldrum belt which constitutes a frictional sink of vorticity.

(2) Much vorticity is exported, particularly during the northern summer, to the continental monsoon systems and " heat lows " which constitute highly effective sinks of vorticity.

(3) A considerable portion of the vorticity is apparently exported around the western extremities of the subtropical anticyclones and supplied to the prevailing westerlies.

It is of interest to note that the monsoon systems and " heat lows " that develop over land in the warm season are characterized by decreasing vorticity along the vertical. Since the air is heated from below, these systems constitute thermal sources and frictional sinks of vorticity. As the warm season develops, these systems approach a balance and become self-supporting.

Related to the monsoon systems are the land and sea breeze, mountain and valley winds, *etc.* We shall not have space to discuss them here.

9. INVERSIONS

As has been shown by Sandström,* energy-producing circulations are possible only when the heat source is at higher pressure than the cold source. Inasmuch as the upper troposphere is steadily losing heat by radiation, a cold source is always available at relatively low pressure, which, together with heat sources at low levels, will maintain circulation with a component across the isentropic surfaces sufficient to compensate for the effect of friction.

During the cold season, the land areas and icefields in northern latitudes act as effective cold sources. Since these sources are situated at high pressure, they will act as brakes on the circulation, with the result that the surface air becomes stagnant and exposed to prolonged cooling by the underlying surface. In this way intense inversion layers develop, forming " cushions " above which the thermally driven and frictionally controlled circulations are found.

Inversion layers of this type (*i.e.*, with lowest temperature at the earth's surface) are typical of the arctic in all seasons and of the polar continental areas during the cold season. A different type of inversion, with lowest temperature at some distance above the surface of the earth, occurs in all seasons in the trade-wind belt, notably in the eastern equatorial quadrant of the subtropical anticyclones. Although the mechanism of the formation and maintenance of this type of inversion has not been fully explored, there can be little doubt of its dependence upon the relative position in the pressure field of the heat and cold sources.

Examples of heat sources constituting brakes in the general circulation are found in the polar stratosphere. As mentioned in Section 2, a pronounced heat source appears to be associated with the ozone layer (*e.g.*, Fig. 1). It will be seen from Fig. 3 that the zonal motion attains a minimum on the equatorial side of this heat source, and a secondary maximum on the poleward side of the sunlit part of the ozone layer. Unfortunately, too little is known of the conditions in the stratosphere to justify further discussion on the circulation at these heights.

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* See V. Bjerknes and collaborators (1933).

10. EFFECT OF MOUNTAIN BARRIERS

As in section 5 we consider the component of the vorticity normal to an isentropic surface. Neglecting for the moment the vorticity of the frictional force, the condition for a stationary vorticity field is obtained from Eq. (7), *viz.*,

$$\nabla \cdot (QV) = Q\frac{\partial V_n}{\partial n}$$

where the term on the left denotes the volume export of absolute vorticity.

Normally, $\partial V_n/\partial n$ is negative on the windward side and positive to the lee of the barrier. Thus, to maintain a steady state, vorticity must be imported to the windward, and exported from the lee side of the barrier. In the absence of the appropriate amount of export and import, vorticity would be consumed to the windward, and produced to the lee of the barrier. Although these influences may be modified by the frictional component, statistical evidence will be presented in section 13 to show that pronounced vorticity sources occur to the lee of the major mountain ranges.

11. THE STATISTICAL MATERIAL

The statistical material presented below has been summarized from a comprehensive investigation of the behaviour of moving cyclones and anticyclones of the northern hemisphere, particularly in relation to the mean circulation, the distribution of land and water, ice fields, mountain barriers, coastal configurations *etc.* The report on this investigation, which will be published later, will contain a critique of the observational material and a description of the method of compilation. Here, it suffices to mention that the basic data were obtained from the *Historical Weather Maps*,* which cover the forty-year period 1899-1939, with one northern hemisphere sea-level chart a day. On these charts the pressure distribution is represented by isobars drawn at intervals of 5 mb.

The positions of all centres of cyclones and anticyclones were evaluated to the nearest degree of latitude and longitude, placed on punched cards, grouped according to seasons, and counted for each five-degree square. To eliminate the effect of the meridional convergence, the frequencies were reduced to a standard area of 100,000 square kilometres, which, approximately, is the area covered by a five-degree square in latitude 70° N.

An event was recorded as cyclogenesis (anticyclogenesis) when and where the first closed cyclonic (anticyclonic) 5-mb isobar appeared, provided that the development could be identified as a cyclone (anticyclone) on the following chart.

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* Published by the U.S. Weather Bureau for the Joint Meteorological Committee.

Since the isobars were drawn at intervals of 5 mb, and since observations are sparse in low latitudes where the pressure gradient is weak, it is evident that many of the small disturbances that occur in the equatorial belt have been eliminated from the *Historical Maps*, with the result that the recorded frequencies of cyclones and anticyclones are far too low in these latitudes. In extratropical latitudes the observational material was found to be adequate except in North Africa, Arabia, parts of Siberia and in the polar region north of about 70° N. The frequencies described below are expressed as a percentage, account being taken of the number of occasions when each five-degree square contained no analysis. All in all, the material comprises 205,534 centres of cyclones, 138,438 centres of anticyclones, 39,691 cases of cyclogenesis, and 26,733 cases of anticyclogenesis.

12. THE ZONAL PATTERN

The mean meridional distribution of the frequencies of cyclogenesis, anticyclogenesis, cyclones and anticyclones is shown in Fig. 13. It will be seen that the primary maximum of frequency of cyclogenesis occurs in latitude 45-50°N. in summer and in latitude 35-40°N. in winter. The corresponding primary maxima of cyclone frequency are found in latitude 60°N. and 55°N. respectively (*i.e.*, about 15 to 20° farther to the north), showing a pronounced poleward tendency of movement. This tendency is even more evident if one considers the variation with latitude of the area between the curves C and CG in Fig. 13.

During the summer season, a secondary maximum of cyclone frequency is found between 30 and 35°N., indicating the mean position of the continental monsoon and heat lows. In this connection it is of interest to note that a small secondary maximum of cyclogenesis is present between 10 and 15°N., reflecting the preferred latitude of formation of tropical disturbances. These disturbances, too, have a northward component of motion and perish in the monsoon lows. Thus, although the monsoon systems are largely thermally driven, as explained in section 8, they derive a certain amount of their vorticity from the disturbances travelling poleward from the intertropical belt.

Both in winter and in summer, indications of secondary maxima in the CG-curves are found north of 60°N., and are apparently due to developments associated with the arctic front.

While the cyclones show a pronounced poleward tendency of movement, the anticyclones show no marked meridional displacement. This is particularly true of the anticyclones that form in middle latitudes ; here the maxima of frequency of anticyclogenesis coincide with those of anticyclones. Particularly during the cold season, a pronounced maximum of anticyclogenesis occurs north of 65°N., and through an independent analysis of individual paths it has been found that these anticyclones almost invariably have a southward movement and tend to merge with the anticyclones of the mid-latitude belt.

The curves shown in Fig. 13 corroborate the theoretical results enunciated in section 7. Thus, while the cyclones find equilibrium in high latitudes, the anticyclones find their stable position either at the pole or in the lower mid-latitude belt.

It is of interest to compare Fig. 13 with the diagrams in section 2. It will be seen that the primary zones of cyclogenesis and anticyclogenesis occur in a régime

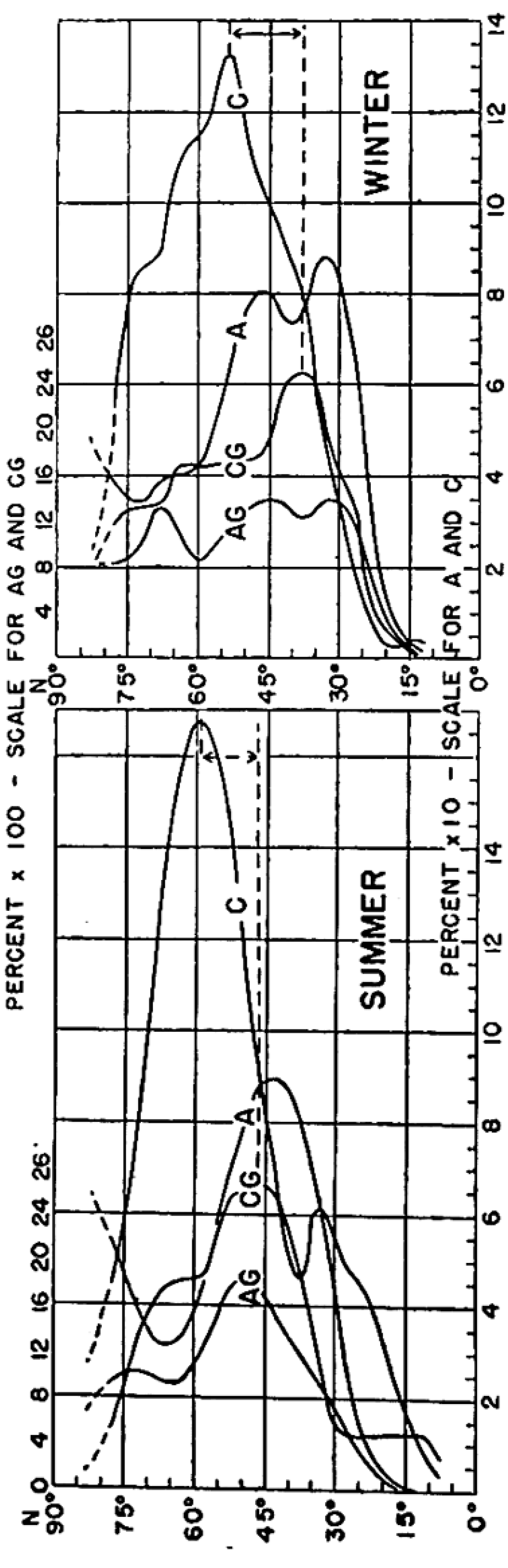

Figure 13. Mean meridional distribution of percentage frequency of anticyclogenesis (AG), cyclogenesis (CG), cyclones (C), and anticyclones (A).

characterized by strong vertical shear of the zonal wind (Figs. 3 and 4), as would be expected according to the theory of dynamic instability referred to in section 1.

The jet stream in the upper troposphere may be compared with a fly-wheel rotating faster than the earth and being exposed to retarding frictional drag from the surrounding air. On the other hand, the belt of cyclogenesis may be identified with the preferred zone of unstable developments, within which conversion of potential energy into kinetic energy takes place. Although the fly-wheel is exposed to frictional drag, it receives impulses from the unstable developments in the mid-latitude belt.

The zonal pattern shown in Fig. 13 is by no means uniform around the world. It will be shown in the following sections that numerous irregularities, due to the topography and the properties of the earth's surface, are superimposed upon the purely zonal system.

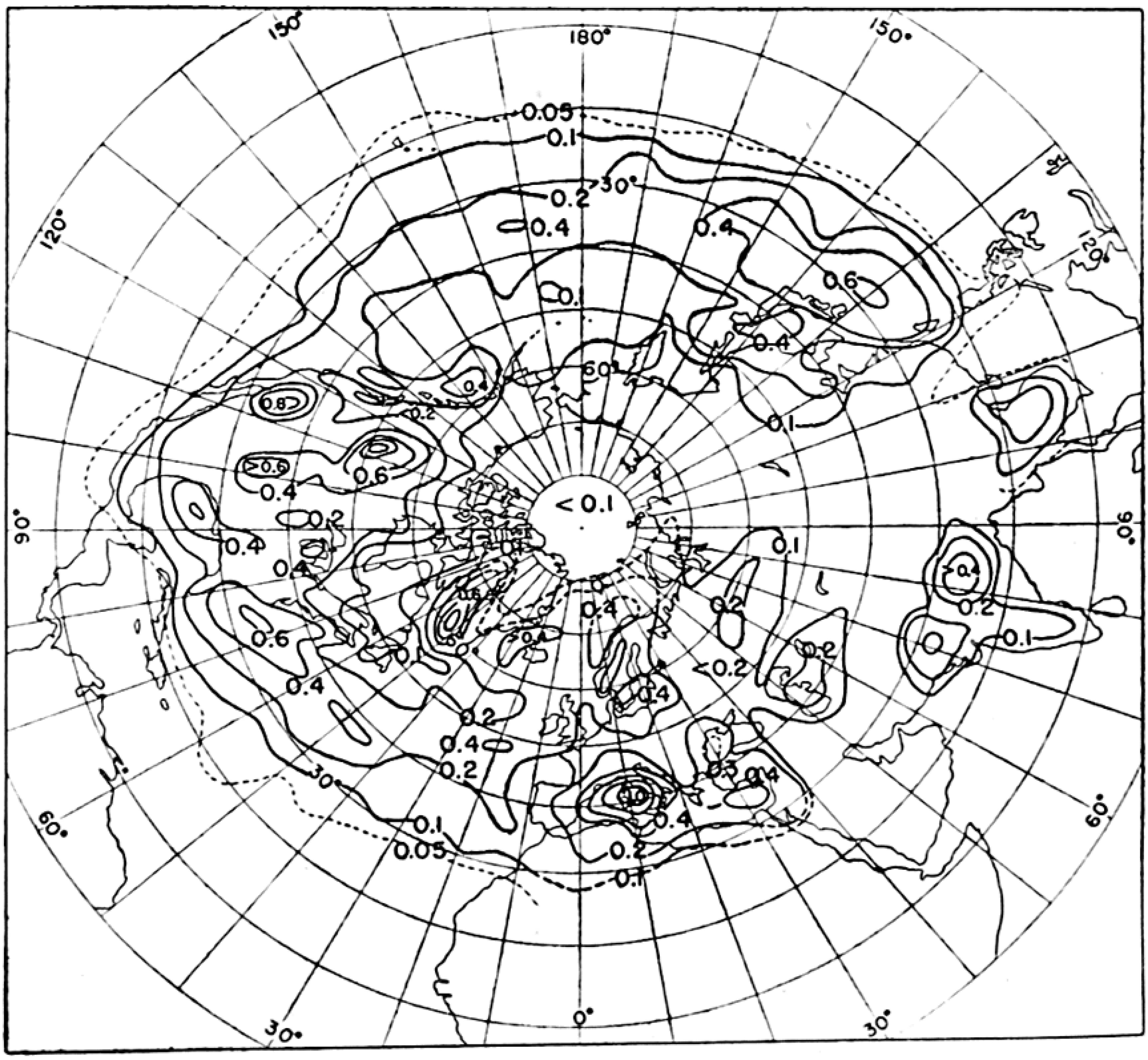

Figure 14. Percentage frequency of occurrence of cyclogenesis in squares of 100,000 km² in winter.

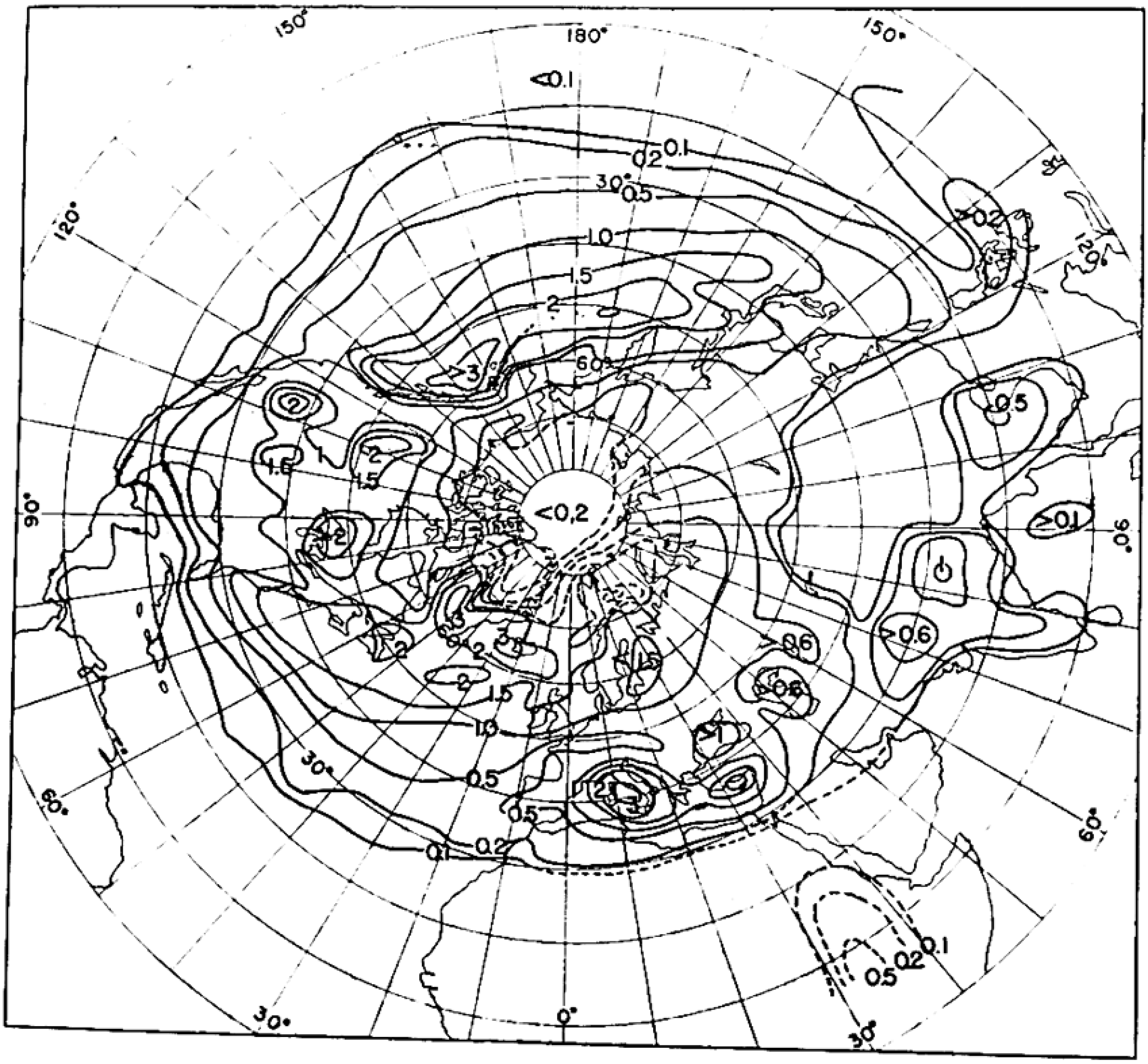

Figure 15. Percentage frequency of cyclone centres in squares of 100,000 km² in winter.

13. THE BEHAVIOUR PATTERN OF CYCLONES

The geographical distribution of the frequencies of cyclogenesis and cyclones is shown in Figs. 14 and 15 for the winter season, and in Figs. 16 and 17 for the summer season. Although a zonal arrangement is clearly evident, the distribution is very uneven, most pronounced maxima and minima occurring in connection with certain geographical features, such as mountain ranges, coastal configurations, bays, and inland water bodies. The following features may be noted.

(1) *The zonal pattern* is indicated most clearly on the North Pacific Ocean (see Fig. 14), where an almost uniform zone of maximum of cyclogenesis extends from west to east in about latitude 30-35°N. The conditions on the Pacific Ocean might, therefore, be taken to indicate the pattern that would exist on a globe uniformly covered by water.

(2) *Mountain ranges* — It will be seen from Figs. 14 and 16 that where the zonal current crosses a major mountain barrier, a maximum of frequency of cyclogenesis occurs in the lee of the barrier. Outstanding examples are the Sierra Nevada, the Colorado, and the Alberta ranges. In the lee of each of these major ranges of the Rocky Mountains a distinct maximum is maintained in all seasons, the intensity being largely independent of the seasonal variation and reversal of the temperature gradient between the North Pacific Ocean and the North American continent. Similar maxima are found to the east of the Appalachian and the Scandinavian mountains and to the east of similarly situated ranges in East Asia.

The influence of mountain ranges is even more apparent in the pattern of frequency of cyclone centres. It will be seen from Figs. 15 and 17 that pronounced maxima are situated in the lee of the aforementioned ranges

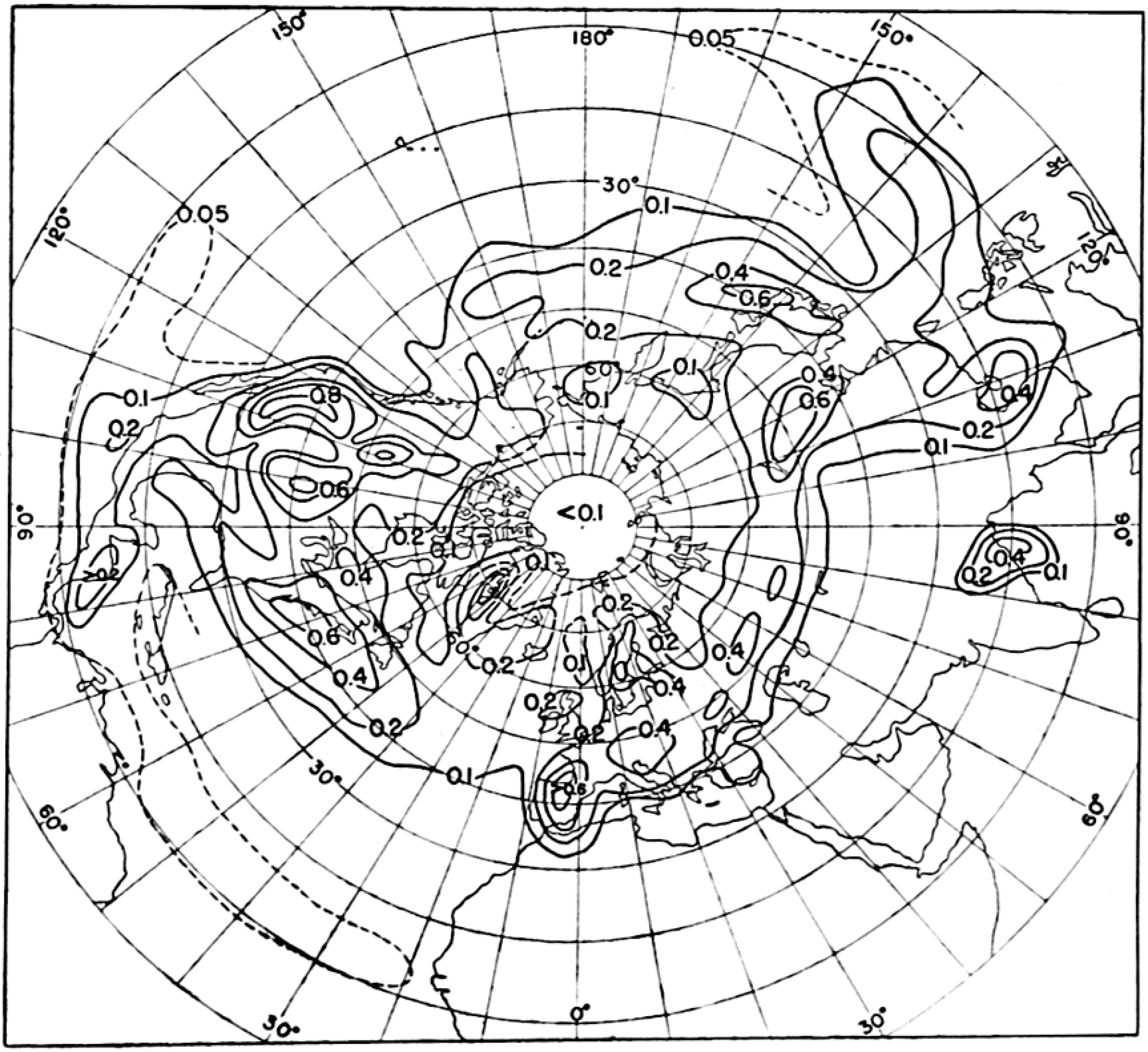

Figure 16. Percentage frequency of occurrence of cyclogenesis in squares of 100,000 km^2 in summer.

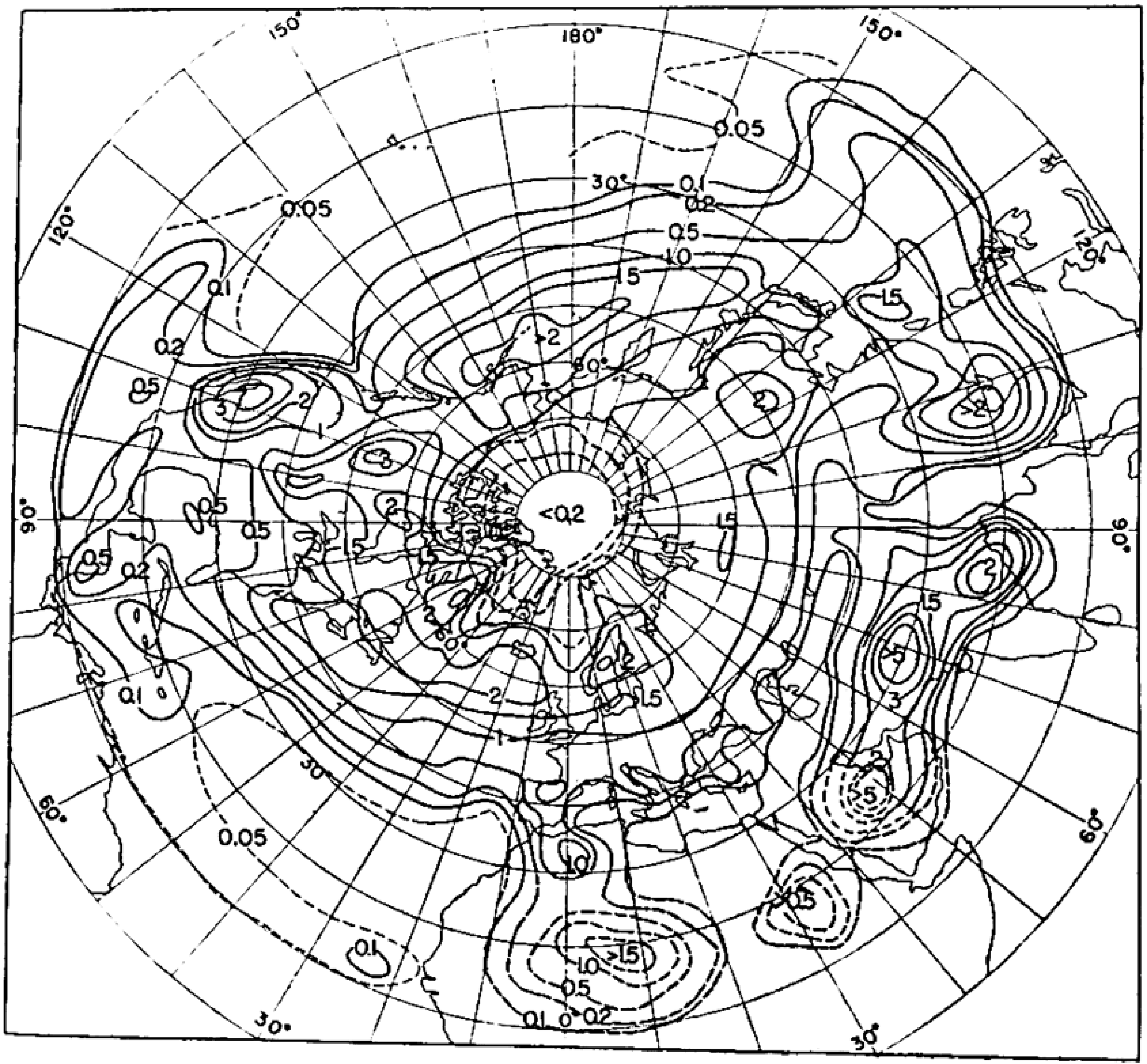

Figure 17. Percentage frequency of cyclone centres in squares of 100,000 km^2 in summer.

of the Rocky Mountains.* Neither of these maxima shows much extension down-wind, indicating that many of the centres that form in this way remain local or are of short life, although some of them develop and travel across the North American continent.

The cyclones that form to leeward of east coast ranges (*i.e.*, the Appalachian and the ranges in East Asia) follow a different pattern. It will be seen from Figs. 14-17 that the maxima of cyclone frequency are found far to the north-east of the maxima of cyclogenesis, thus indicating that these cyclones are long-lived and move with considerable speed.

The foregoing examples suffice to show that the theoretical results developed in section 10 are essentially correct. They might also be taken to indicate that mountain building has exerted a profound influence on past climatic variations.

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* In summer, the Sierra maximum merges with the "heat low" and the frequency is greatly increased.

(3) *Bays and inland water bodies* — It will be seen from Fig. 14 that distinct maxima of cyclogenesis occur during the cold season over all major inland water bodies. The most outstanding example is found in the Mediterranean area where there are two maxima, separated by the Italian peninsula. Similar maxima are found over the Great Lakes of North America, the Baltic, the Black Sea, the Caspian Sea and the Aral Sea. The influence of inland water bodies is even more pronounced in the frequency of cyclone centres (Fig. 15). In the warm season (Figs. 16 and 17), when these water bodies are colder than the surrounding land, distinct minima of frequency of cyclogenesis and cyclone centres occur over these water bodies and are replaced by maxima of anticyclonic characteristics.

The influence of warm water surrounded (wholly or partly) by cold land is noticeable also in connection with the bays on the fringe of the arctic ice fields. Examples are the Gulf of Alaska (winter), Baffin Bay and Davis Strait (winter and summer), Denmark Strait (mostly winter), the waters between Norway and Spitsbergen, and the Barents Sea.

Although many of these local systems are of small extent and intensity, their well-defined features and isolated occurrence probably affords the most convincing evidence in support of the theory of vorticity transfer, as set out in sections 5 and 6.

(4) *The subpolar semipermanent lows* — The large areas of maximum cyclone frequency over the northern parts of the North Pacific and North Atlantic (Figs. 15 and 17) are of interest, particularly since they coincide with areas of low frequency of cyclogenesis. As explained in sections 6 and 7, these semipermanent lows are maintained partly by import of vorticity from the adjacent cold sources and partly by influx of travelling cyclones from the mid-latitude belt.

On account of the complex topography of the Atlantic polar sector, the pattern of cyclone frequency in this region is more complex than that of the Pacific sector. During the cold season, the pattern follows closely the configuration of the border of arctic ice fields and land masses. In summer, when the main temperature contrast is found along the border between cold water and warm land, the Barents Sea maximum is absent, and instead, an irregular belt of high cyclone frequency extends across both continents, broadly in latitude 60°N.

(5) *The monsoon systems* — It will be seen from Fig. 17 that vast areas with high cyclone frequency occur over land in subtropical latitudes in the warm season. In South Asia, the frequency of cyclogenesis in and near the areas of maximum cyclone frequency is very low (generally less than 0·1 per cent), showing that the monsoon system, at the height of the season, is not primarily maintained by influx of perturbations from local or nearby sources. In spring and autumn, when the basic monsoon system is weak, areas of appreciable frequency of cyclogenesis are formed in these regions, and it appears that the monsoon system in spring feeds on the vorticity of the perturbations until it becomes firmly established as a more or less self-supporting local thermal circulation (*i.e.*, thermal source and

frictional sink). In the autumn, the life of the monsoon low is prolonged through the influx of vorticity from nearby regions of cyclogenesis. However, even in summer, the amount of vorticity supplied from nearby sources of cyclogenesis, notably from the Bay of Bengal (see Fig. 16), is appreciable.

In one respect the semipermanent subpolar cyclone systems on the North Atlantic and North Pacific oceans resemble the summer monsoon systems in South Asia, namely in that they are maintained partly through local thermal circulations and partly through influx of perturbations from neighbouring regions. The analogy is particularly striking in spring and autumn when the basic monsoon system is weak.

Referring again to Fig. 17, it is of interest to note the high frequency of cyclone centres to the south of California and the similar, although lesser, maximum to the west of Africa. Being situated over relatively cold water, these systems cannot be maintained in the same manner as the monsoon lows. They appear to be the frictional sinks that take up the vorticity exported from the trade-wind source further to the north (see section 8) ; they represent an exaggeration of the normal doldrum circulation.

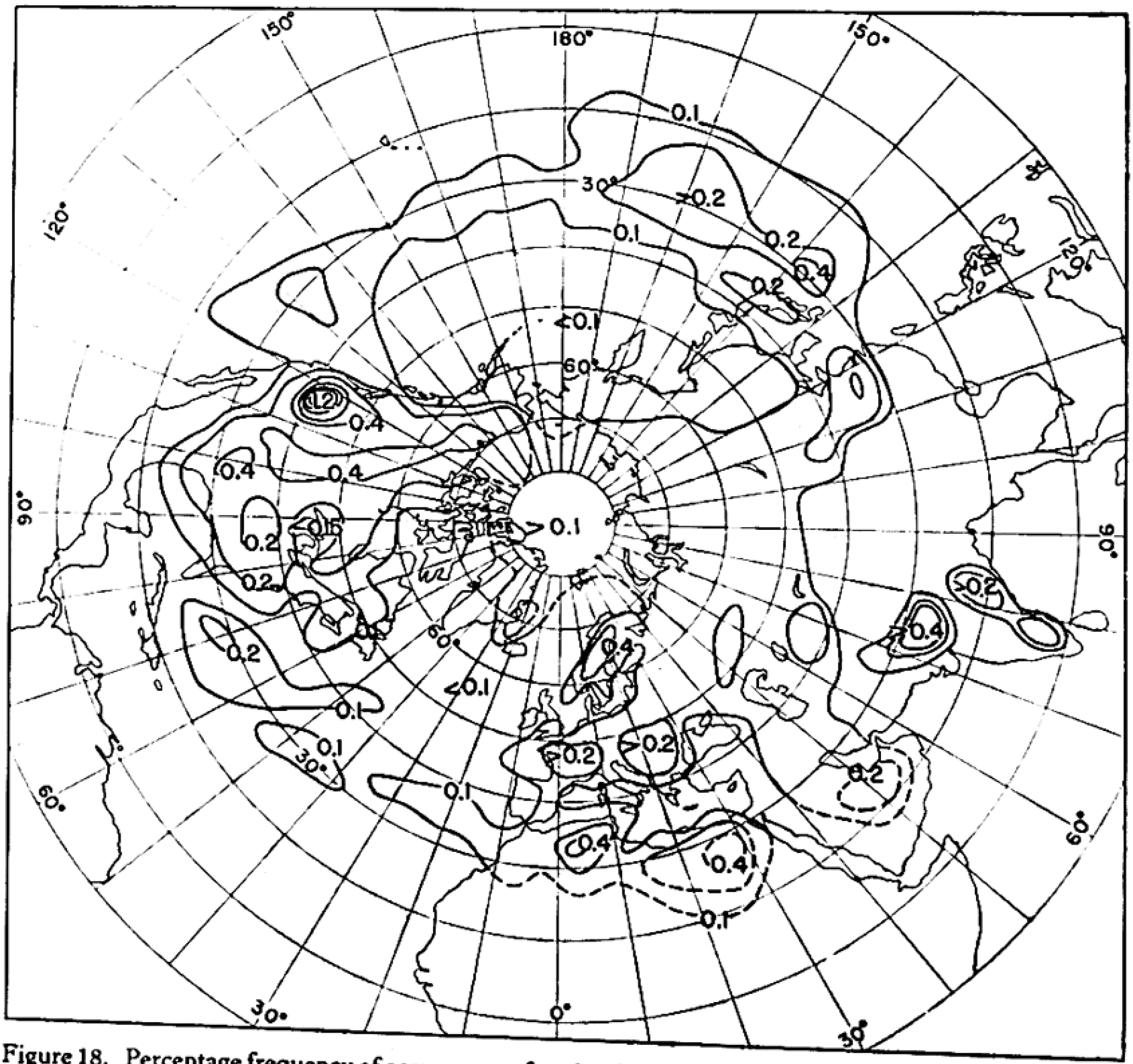

Figure 18. Percentage frequency of occurrence of anticyclogenesis in squares of 100,000 km² in winter.

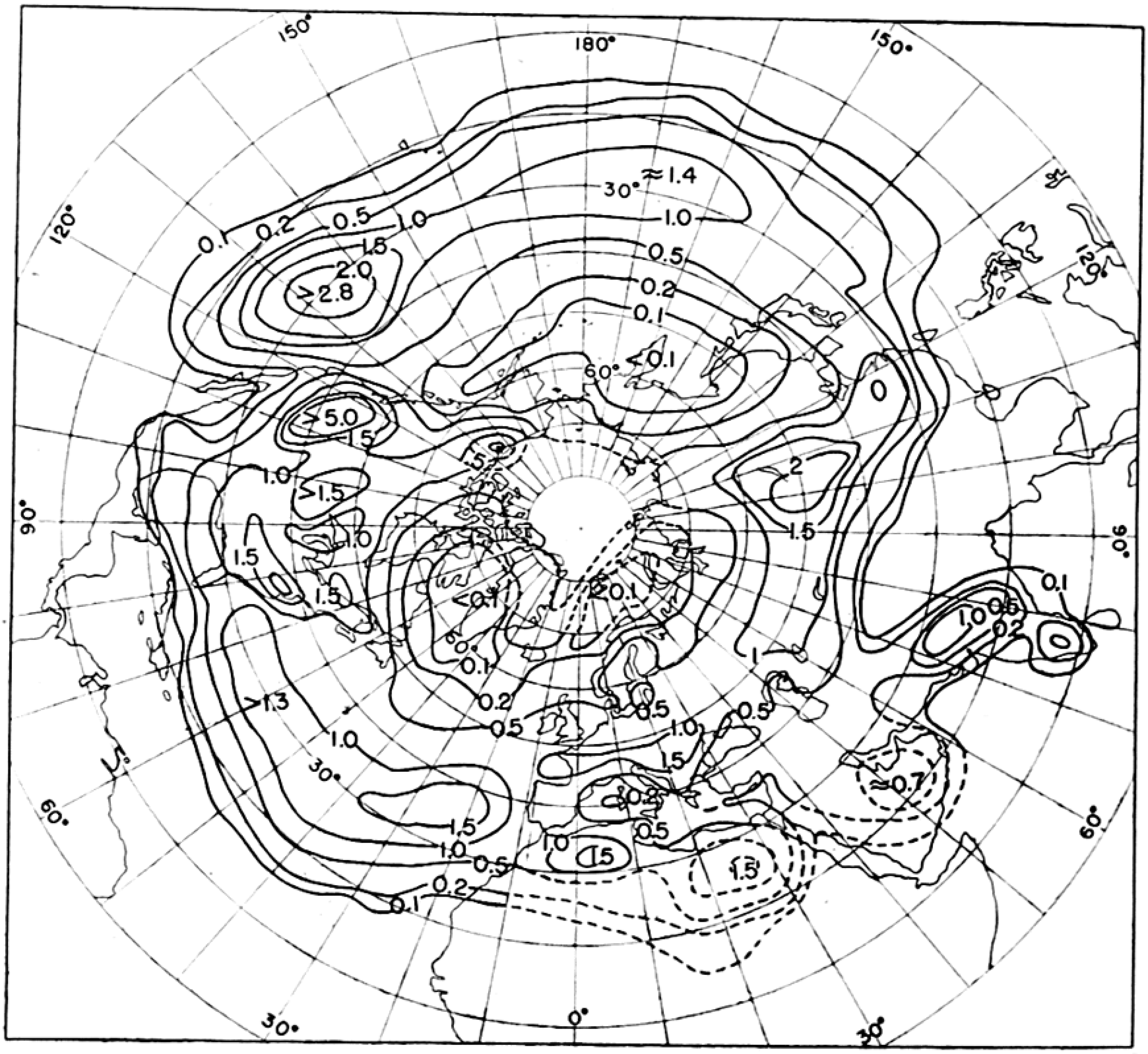

Figure 19. Percentage frequency of centres of anticyclones in squares of 100,000 km² in winter.

14. THE BEHAVIOUR PATTERN OF ANTICYCLONES

The geographical distribution of the frequencies of anticyclogenesis and anticyclonic centres is shown in Figs. 18-21 for winter and summer.

The zonal pattern is most clearly indicated on the North Pacific in winter (Figs. 18 and 19), where an almost uniform belt of maximum frequency of anticyclogenesis is situated along the 30th parallel. The corresponding maximum of frequency of anticyclonic centres occurs also in 30°N., showing that there is no appreciable tendency of meridional movement.

Superimposed upon the zonal pattern is a number of local maxima and minima associated with such geographical features as mountain barriers, inland water bodies, coastal configurations, and bays on the fringe of the arctic.

In regard to mountain influences, it suffices to remark that plateaux rather than ranges appear to be the dominating factor in shaping the winter patterns of anticyclone frequency (*cf.* Figs 14 and 18, and 15 and 19).

In regard to the influences of inland water bodies, it is of interest to note that maxima occur in summer (when the water is colder than the surrounding land) and minima in winter (when the temperature difference is reversed). Over the bays on the fringe of the arctic, minima occur in all seasons.

15. Ducts of perturbations

The frequency charts described in the foregoing sections include travelling and short-lived pressure centres as well as quasi-stationary and semipermanent ones, and the over-all patterns of distribution are largely determined by the latter category (*e.g.*, the Icelandic low, the Aleutian low, the subtropical highs, the monsoon systems, *etc.*). It would obviously be of considerable interest to examine separately the pattern of travelling disturbances. It is, however, extremely difficult to define the two categories separately in any objective manner, and even if a suitable definition

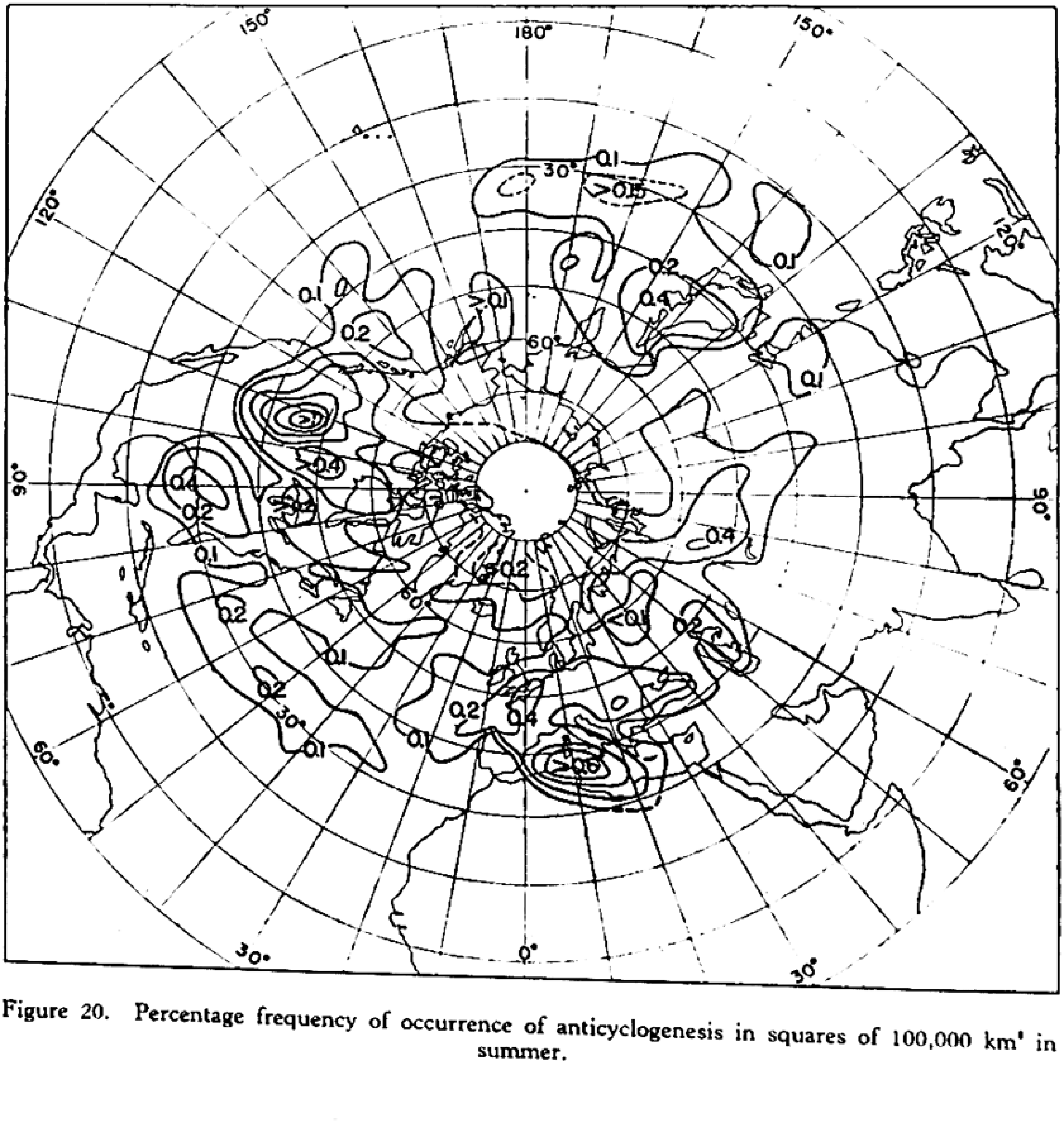

Figure 20. Percentage frequency of occurrence of anticyclogenesis in squares of 100,000 km² in summer.

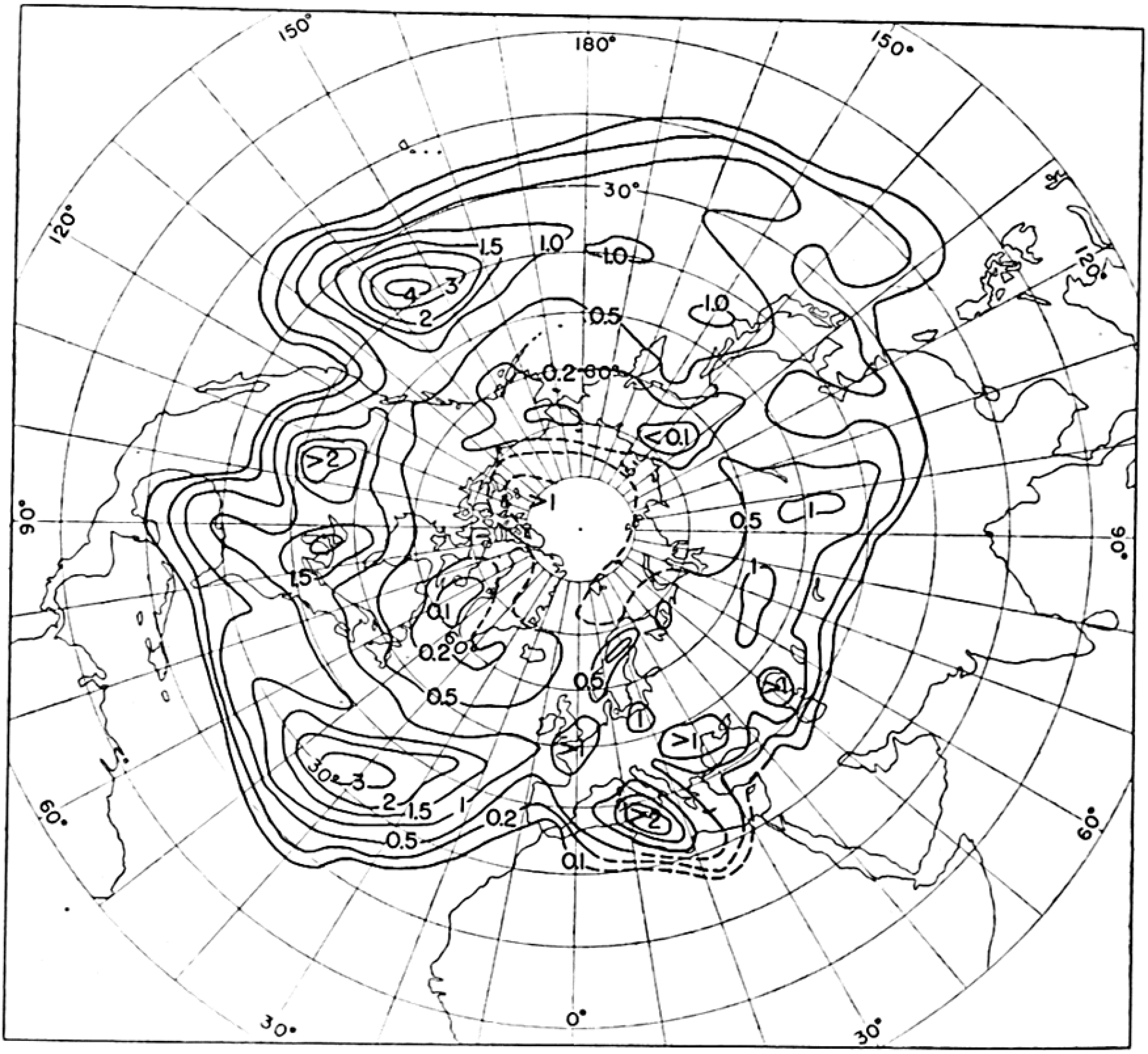

Figure 21. Percentage frequency of centres of anticyclones in squares of 100,000 km^2 in summer.

could be provided, the statistical work involved in charting their patterns of behaviour would be almost prohibitive, even with the considerable facilities at the disposal of the writer.

A measure of the rate of alternation between cyclonic and anticyclonic centres is, however, readily obtained. If F_C and F_A denote the percentage frequencies of occurrences of cyclone and anticyclone centres, respectively in a given square, the ratio F_C/F_A (when $F_C < F_A$), or the ratio F_A/F_C (when $F_C > F_A$) may be taken to express this rate of alternation. In regions occupied by the semipermanent pressure systems, this ratio will be small, and along the lanes where travelling cyclones and anticyclones alternate, it will be large ; if the cyclones and anticyclones occur with equal frequency, the ratio will be unity, or one hundred per cent. Through the introduction of this ratio, the effect of the semipermanent pressure systems will be largely eliminated, and the remainder may be taken to represent, in a broad sense, the statistical picture of what may be called the travelling perturbations superimposed upon the basic circulation.

The above-mentioned ratio has been evaluated from the frequency charts and the results are shown in Figs. 22 and 23 for winter and summer, respectively.

It is convenient to discuss first the summer season (Fig. 23) since the heat equator then is well to the north of the geographical equator.

It will be seen that the monsoon systems (*cf.* Fig. 17) of southern Asia have been almost completely eliminated, while the extratropical semipermanent systems appear as distinct minima (generally less than 10 per cent). The most striking feature in Fig. 23 is a well-defined narrow lane of maximum rate of alternation on the Pacific Ocean, beginning to the east of Guam, extending first north-westward, then recurving between 20 and 25°N., and continuing across the Pacific into North America at the southern border of Canada. A similar and almost identically shaped

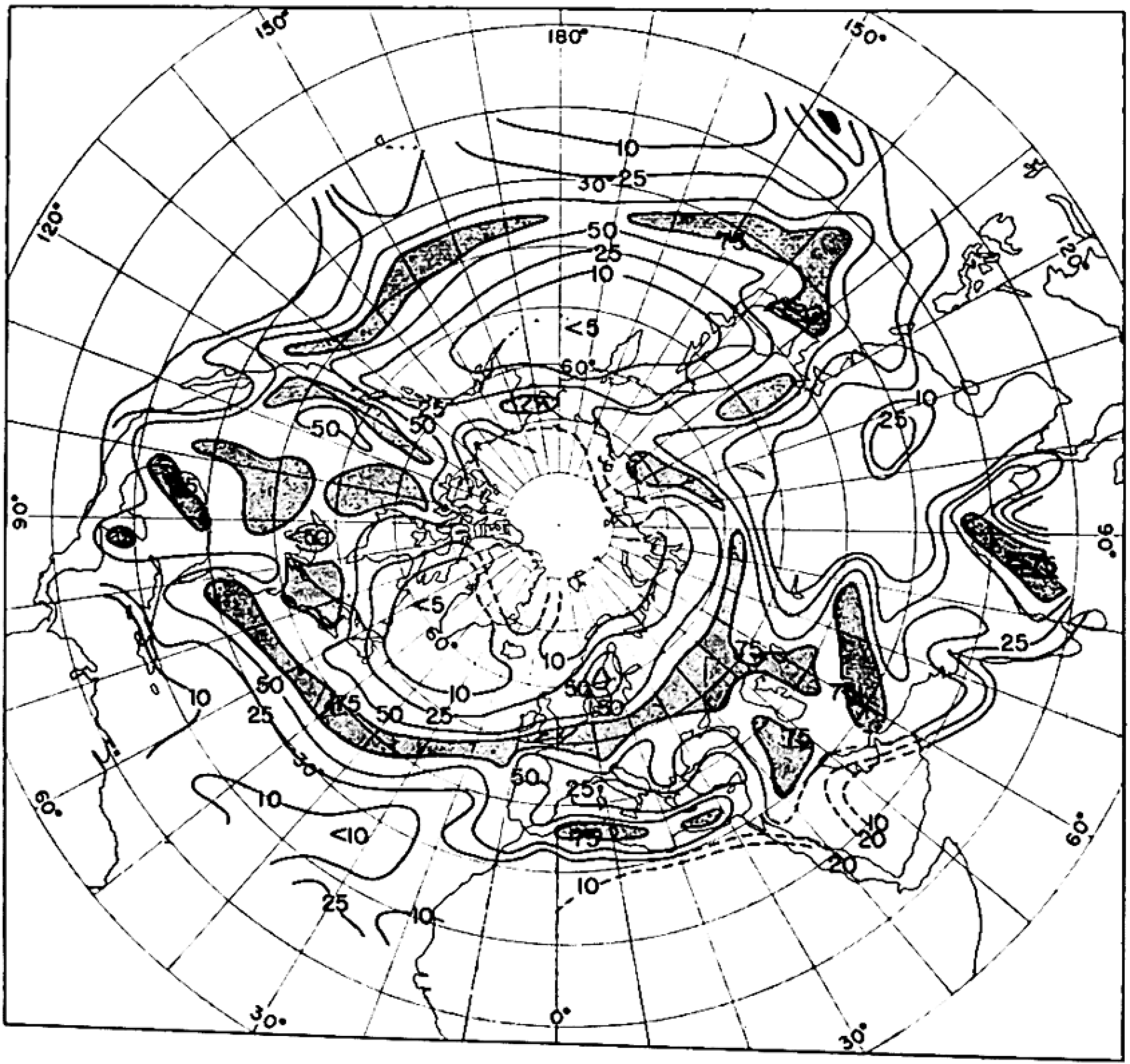

Figure 22. Rate of alternation (per cent) between cyclones and anticyclones in winter, indicating the distribution of travelling disturbances.

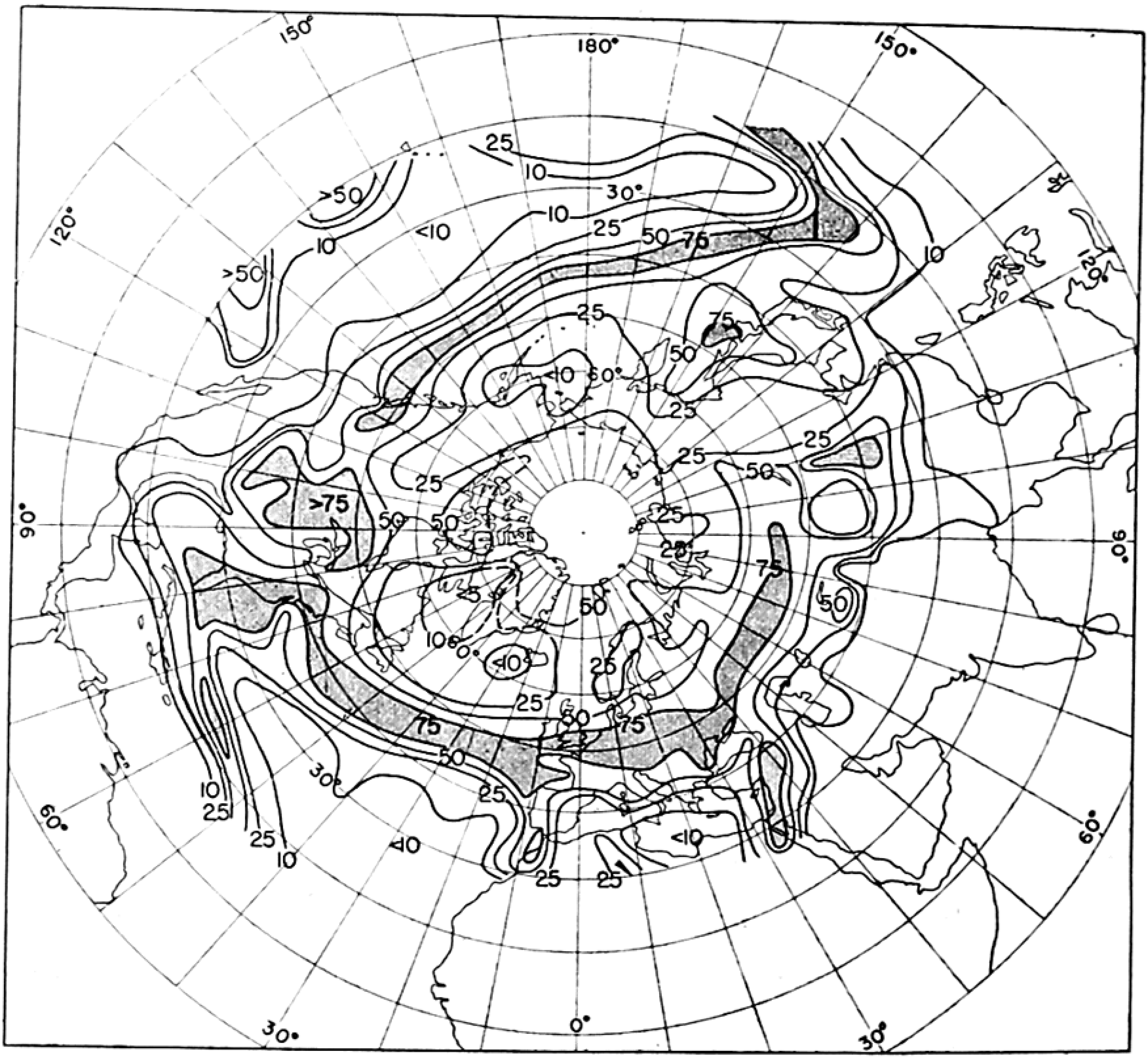

Figure 23. Rate of alternation (per cent) between cyclones and anticyclones in summer, indicating the distribution of travelling disturbances.

lane of perturbations is found in the North Atlantic area, extending deep into the Eurasian continent.

In winter (Fig. 22) the conditions are largely the same as in summer, except (a) that the intertropical branches of the lanes are less distinct ; (b) that a merging subpolar branch to the lanes tends to appear in East Asia and also in the north-eastern part of North America ; and (c) that the continental patterns are somewhat disrupted by mountain ranges, inland water bodies, *etc.*

It should be emphasized here that the synoptic charts from which the frequencies have been extracted represent the pressure distribution by isobars at intervals of 5 mb. In low latitudes, the pressure gradients are usually so weak that numerous centres of highs and lows may exist without appearing in the five-millibar isobars, and numerous perturbations (known as " easterly waves ") move along the major current without forming closed systems. Thus the frequencies of the separate occurrence of cyclones and anticyclones, shown in Figs. 15, 17, 19 and 21, are likely to be far too low as a measure of the number of perturbations that travel in the trade wind belt. However, since Figs. 22 and 23 represent the ratio of such frequencies, the effect is largely eliminated. It appears, therefore, that numerous small perturbations originate in the equatorial belt and travel along the aforementioned lanes into the prevailing westerlies where they, as a result of unstable rearrangements of potential and kinetic energy, develop into extratropical cyclones and anticyclones.

The lanes shown in Figs. 22 and 23 may be regarded as the *ducts* along which the perturbations normally travel. On the poleward and westward side of these ducts, the frequency of cyclones is larger than that of anticyclones, while the reverse is true on the opposite side of the duct. It follows then that cyclonic vorticity, as it develops while the perturbations travel, must be " sprayed " from the duct into the subpolar semipermanent cyclonic systems which constitute the sinks of relative vorticity, while anticyclonic vorticity must be " sprayed " in the opposite direction

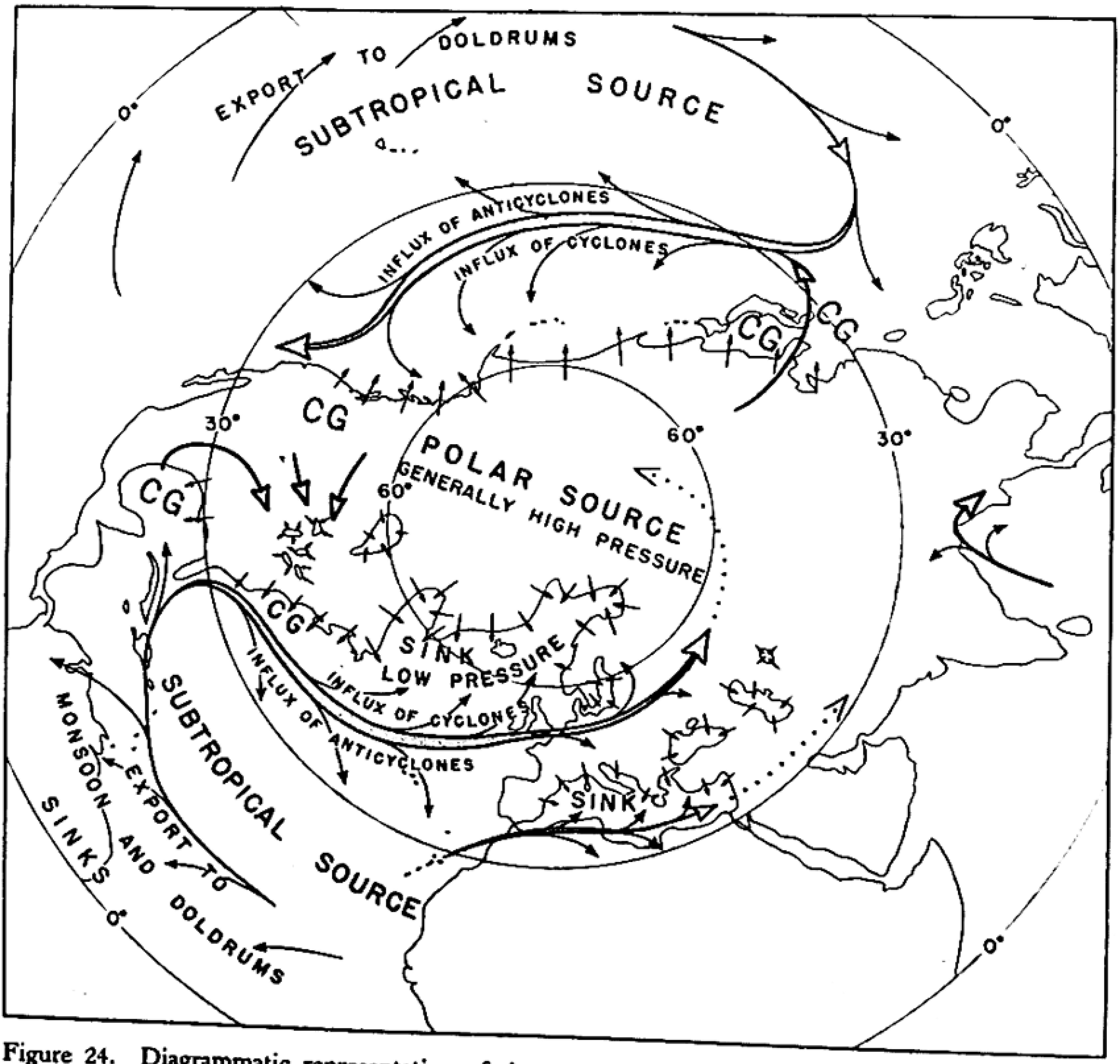

Figure 24. Diagrammatic representation of the essential features of the circulation of the lower troposphere in winter. The contours indicate the distribution of water *versus* land and ice. Short straight arrows indicate direction of downward slope of isentropic surfaces.

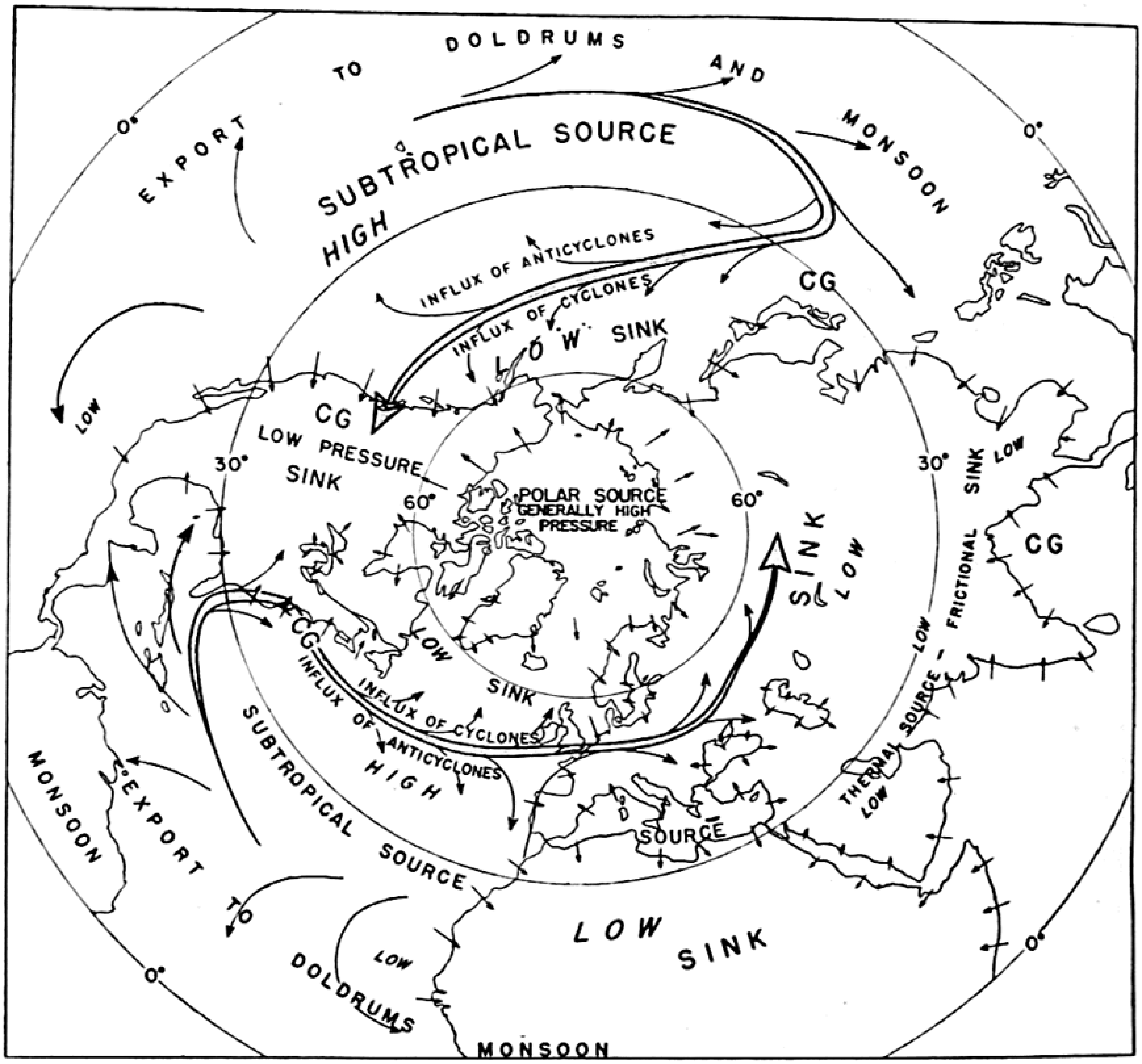

Figure 25. Diagrammatic representation of the essential features of the circulation of the lower troposphere in summer. Short straight arrows indicate direction of downward slope of isentropic surfaces.

into the subtropical anticyclones which constitute the sinks of relative anticyclonic vorticity. Similarly, cyclonic vorticity is "sprayed" from the tropical and subtropical branch of the duct into the summer monsoon systems and into the doldrums.

16. Circulation models for the lower troposphere

The essential results of the foregoing discussion are summarized in Figs. 24 and 25, for winter and summer, respectively. From the broadest point of view three principal régimes may be identified, *viz.*,

(1) A low-latitude circulation which is mainly thermal. Here the air is heated in a field where the absolute vorticity decreases in the direction of increasing entropy, with the result that the thermal component of the vorticity source is positive. The circulation near the earth's surface being cyclonic, the vorticity of the frictional force will be anticyclonic, with the result that the thermal and the frictional components

tend to balance. The low-latitude systems (which essentially are of the monsoon type) tend, therefore, to become steady systems.

(2) A high-latitude circulation which is essentially thermal. Here the air is cooled in a field where the absolute vorticity increases with increasing entropy. The thermal and the frictional components of the vorticity source are then both positive, with the result that stationary conditions can be established only through export to other regions.

(3) A mid-latitude circulation dominated by travelling cyclones and anticyclones in which a separation of vorticity occurs, such that the cyclonic eddies are shed towards higher, and anticyclonic eddies towards lower latitudes, thus contributing towards the maintenance of the semipermanent subpolar belt of low pressure and of the subtropical belt of high pressure.

(4) These major systems are modified in considerable detail by the influences of mountain ranges, inland water bodies, *etc.*

(5) Superimposed upon the zonal arrangement is the annual variation in heating and cooling which, broadly speaking, leads to zonal transfers of vorticity from continents to oceans in winter, and from oceans to continents in summer.

(6) During the cold season the surface of the land and snow fields in high and middle latitudes act as cold sources of appreciable intensity. Since these sources normally are situated at higher pressure than the adjacent heat sources, the circulation is hindered and extensive inversion layers with stagnant air develop at the ground.

The writer is aware that he has paid little attention to the conditions of the upper troposphere and lower stratosphere, and in particular to the heat and cold sources associated with the varying composition of these layers (cloud, water vapour, ozone, *etc.*). Our knowledge of these conditions, however, is so meagre that an attempt at establishing a vorticity budget for these layers must wait for suitable observations to become available.

ACKNOWLEDGMENTS

The writer wishes to express his indebtedness to Brigadier General D. N. Yates for the generous facilities provided for this investigation ; to Dr. G. P. Cressman for analyzing the synoptic situations referred to in Section 4 ; and to Majors R. C. Bundgaard and A. F. Gustafson, Captains H. A. Bedient and L. G. Starrett, Messrs. H. S. Appleman, R. H. Ferrell and J. R. DeHart for their assistance in collecting and preparing the statistical data.

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The cause of the general circulation of the atmosphere

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Summary

The aim is to explain the fundamental causes of large-scale atmospheric behaviour and to deduce the observed mean flow pattern directly from the equations of motion, *etc.*, without additional hypotheses. It is inferred from empirical evidence that there exists a poleward and upward transfer of heat and a transfer of angular momentum polewards over most of the earth's surface but equatorwards in high latitudes. The possibility of the main transfer being the result of steady currents, including meridional circulations, is considered and rejected ; it is concluded that large-scale turbulence plays a fundamental role in the mechanism of the atmospheric " engine." The problem therefore is to explain *why* large-scale turbulence develops and to derive its properties with regard to transfer of heat and angular momentum. From earlier work on the stability of baroclinic motion it is concluded that the mean flow is always unstable and must continually generate turbulence. The mechanism by which instability is continually re-established, and turbulent flow consequently maintained, is described and its intimate relation to the observed heat transfer deduced. The transfer of angular momentum is a secondary feature associated with the " constraints," to which the complete set of equations and boundary conditions are equivalent, which modify the form of the overturning process represented by the growth of turbulent disturbances. Difficulties in computing this transfer theoretically arise from the fact that angular momentum is not even approximately conserved during motion. The hypothesis that the vertical component of absolute vorticity is conserved is rejected both for theoretical reasons and because it leads to incorrect results in middle latitudes. It is suggested however that this hypothesis may be valid for approximate calculations near the poles and near the equator because the motion is more nearly barotropic. Transfer behaviour in middle latitudes is deduced from a consideration of the structure of growing disturbances. Surface friction plays an essential part in the circulation in that it limits the amplitude both of individual disturbances and of the zonal currents resulting from angular momentum transfer. The mechanism by which its effect is "geared" to motion above the friction layer is described and it is inferred that *driven* meridional circulations must exist. These meridional circulations are secondary effects so far as heat transfer is concerned. They are consistent with observed precipitation belts and are slow enough to allow maintenance of the observed lapse rate by radiative cooling in the subsiding currents.

1. Heat transfer on a large scale

The present paper is concerned to explain the observed eddy-transfer of heat and angular momentum between latitudes and incidentally to explain why there is any atmospheric " circulation " at all. The term *circulation* is to some extent misleading in that it suggests that the main heat and momentum balance is maintained by quasi-steady currents whereas according to the views expressed here these currents are secondary features. If this is understood there is no objection to using the conventional term.

It is convenient to commence by examining the facts about heat and angular momentum transfer which have to be explained. Let us consider first heat transport between different zones (bounded by lines of latitude).

(1) The calculations of Simpson (1928) and others indicate that the different zones of the atmosphere are not in radiative equilibrium. Poleward of about latitude 35° there is a net annual loss of heat by radiation ; equatorward of this latitude there is a compensating net annual gain of heat. Although the calculations are necessarily rough there is no reasonable doubt about the general nature of the result. We must infer a net pole-

ward transfer of heat and the only plausible mechanism would appear to be advection by air and/or ocean currents. Since the surface ocean currents are driven by the atmospheric circulation a plausible inference is that at least part and possibly the major part of the heat transfer is by air currents.

(2) Calculations of radiative transfer in the vertical indicate that on the average the free atmosphere below the tropopause is cooling, owing to radiative processes alone, at a rate of something like 1°C per day. Recent computations, not yet published, by Howell at Imperial College, of the change of potential temperature along (3-dimensional) trajectories in the middle troposphere have given independent confirmation of this result. We must infer a net upward transfer of heat and the only plausible mechanism would appear to be advection by air currents. This would include, of course, heat transfer by ordinary convection but this is not necessarily the sole mechanism.

Combining these results it would appear that we have to account for a poleward and upward transfer of heat. The most plausible hypothesis is that this transfer is effected by the motion of relatively potentially warm air polewards and upwards while relatively potentially cold air moves equatorwards and downwards to take its place. Since we have already taken radiation into account we must *logically* regard this process as adiabatic even though in practice the adiabatic and radiative changes are not completely separated. We then notice an interesting feature. For simplicity we shall consider the air to be unsaturated so that in an adiabatic process entropy is conserved. (Condensation is certainly an important atmospheric process but to take it into account merely complicates the argument without affecting the general nature of the result.) Hence we may regard the postulated transfer as a transfer of entropy $\Delta\phi$. If T_1 is the mean temperature at the low-level low-latitude end and T_2 the mean temperature at the high-level high-latitude end then statistical equilibrium requires that the net incoming radiation in low latitudes is proportional to $T_1\Delta\phi$ while the net outgoing radiation in high latitudes is proportional to $T_2\Delta\phi$. Since $T_1 > T_2$ more heat is received than is re-radiated. Thus our supposed mechanism implies that the atmosphere behaves as an engine and does work proportional to $(T_1 - T_2)\ \Delta\phi$. Now by hypothesis statistical equilibrium is maintained so there is no ultimate change in either the potential or internal energy of the atmosphere : the work done must manifest itself as a growth of kinetic energy to balance statistically the loss of kinetic energy (*i.e.*, its conversion into heat which is then radiated away) by frictional forces. In the last analysis there is a complete radiation balance but with the mechanism we have supposed kinetic energy is maintained at a finite level (*i.e.*, there must be a "circulation" of some kind), otherwise there would be no frictional forces. (See Fig. 1)

The inferred mechanism makes it clear *how* an atmospheric circulation is maintained — it is a necessary consequence of the existence of a heat transport in a certain direction. What we have to explain is why such a heat transport exists, *i.e.*, why the atmosphere is not in radiative equilibrium. Before considering the cause of this heat transport we may note that not only is the sink colder than the source, as required by the Second Law of Thermodynamics, but it is also at a higher

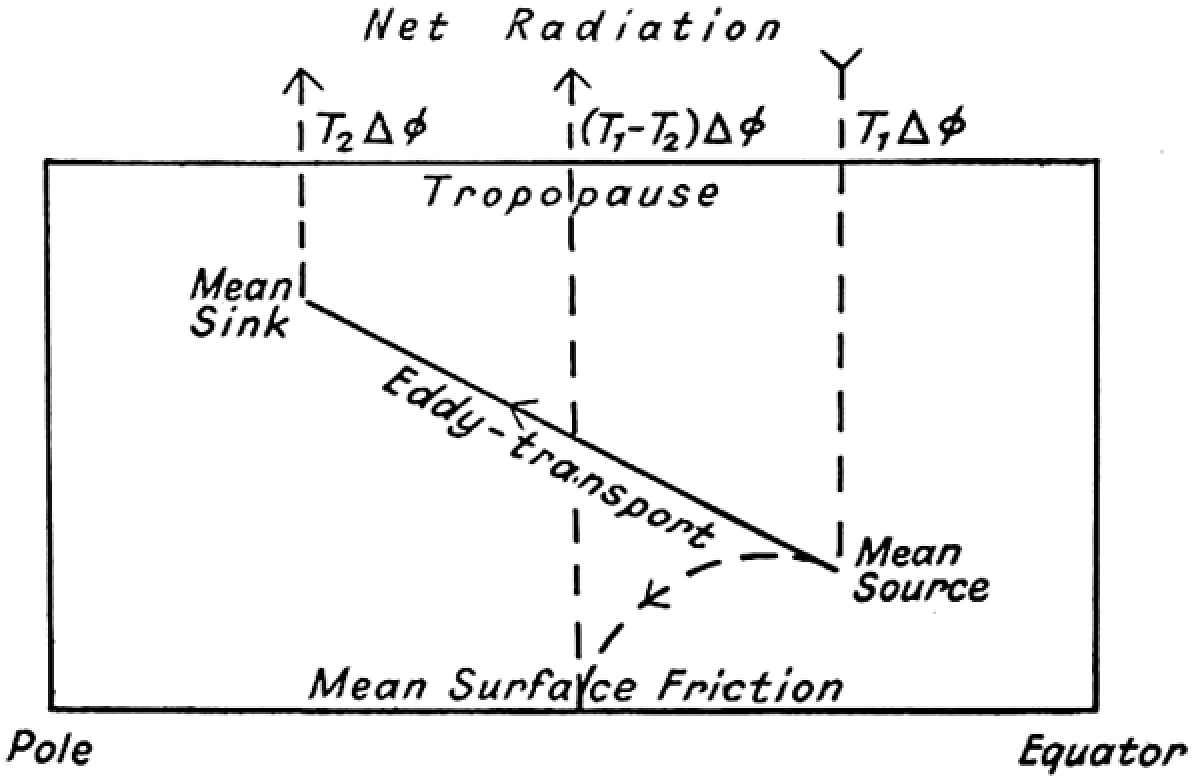

Figure 1. Heat and energy transfer in the atmosphere. The straight dashed lines indicate radiative transfer. The curved dashed line indicates generation of kinetic energy through the working of the atmospheric " engine." Bodily (large-scale eddy) transport of heat is indicated by a full line. The figure represents a section in a meridional plane.

level. Various writers (see *e.g.* V. Bjerknes *et al* 1934, p. 200) have attempted to show that this is a necessary feature for the maintenance of a steady circulation. We shall see that it is also necessary for the maintenance of large-scale turbulence. Thus this feature is almost certainly necessary for the maintenance of any circulation at all and gives no clue to its nature. Nor should we infer that the existence of a circulation is accidental in so far as it depends on the relative levels of source and sink. The genesis of source and sink regions (*i.e.*, regions not in radiative equilibrium) is the *result* of an initial circulation and the mean circulation pattern adjusts itself to maintain statistically the heat and cold sources at levels consistent with its own maintenance.

2. WHY TURBULENT MOTION MUST BE INFERRED

Let us consider whether the atmospheric circulation could be maintained by steady currents. In the northern hemisphere the observed mean currents are somewhat irregular and presumably related to the topography and the distribution of land and sea. In the southern hemisphere there is still a circulation but this appears to be more nearly zonal and moreover the zonal currents are intensified. The inference is that the distribution of land and sea plays no *fundamental* part in maintaining the circulation and that the cause of the circulation must be revealed if we consider an ideal earth with a uniform surface. Then the only *steady* currents allowed by symmetry, in addition to zonal motion, are meridional circulations. The zonal motion does no work and must be a secondary feature. Let us consider the possibility of steady meridional circulations. These could effect the required heat transfer and calculation shows (V. Bjerknes *et al* 1934, p. 732) that the currents required are too small for direct measurement to give conclusive evidence for or against. The hypothesis that such currents exist is attractive, because if the earth did not rotate (while the sun revolved round the earth) there is little doubt that such currents would be generated. However the distribution of vertical motion does not fit with observed precipitation belts and when we take the earth's rotation into account we encounter theoretical difficulties. If we neglect frictional forces large-scale meridional circulations are impossible. This is because the angular momentum of each ring of air is necessarily conserved during its motion. Any motion in a meridional plane would cease when the thermal wind equation is satisfied: the temperature contrast between pole and equator is too small to allow the air to traverse more than a small part of its hypothetical circuit. When the thermal wind equation is satisfied we have a state of zonal motion in equilibrium and moreover (except *possibly* in very narrow belts on the equatorial side of jet streams) in stable equilibrium from the point of view of displacements in a *meridional* plane (Eady 1949, p. 51). Frictionless meridional circulations fail because they are associated with much too large a transfer of angular momentum. Meridional circulations *could* be maintained if the increasing angular momentum difference between poleward and equatorward currents were continually being destroyed by frictional forces, the only plausible mechanism being the transfer of angular momentum by turbulence, *i.e.*, eddy-viscosity. Turbulence is therefore a necessary feature of atmospheric motion whether the heat transfer occurs directly as a result of it or whether the heat transfer is by meridional circulations. Any theory of the atmospheric circulation must be based on a theory of (large-scale) atmospheric turbulence. Moreover this theory must go deeper than most existing theories of turbulence. It is futile to beg the question by introducing hypothetical coefficients of eddy-transfer. We have not only to establish the cause of the turbulence but also to derive from first principles its properties with regard to transfer of heat, angular momentum, *etc.* As we shall see, these properties are not properties of turbulent motion in general but depend on the mechanism by which turbulence is generated and maintained. We shall find that the main heat transfer is direct and necessarily related to the maintenance of turbulence. Meridional circulations do exist and are related to turbulent transfer of angular momentum in the manner described above but they are secondary features different in structure from the hypothetical circulations postulated above and much feebler.

3. THE CAUSE OF LARGE-SCALE TURBULENCE

We can infer, as a theoretical necessity, that atmospheric motion is turbulent if we can show that any hypothetical laminar motion is unstable. Then any small irregularity must lead to the generation of turbulent disturbances. The statistical effect of these disturbances is an alteration in the mean flow and the new flow pattern is possible only because of the transfer properties of turbulence. As friction damps out the turbulent disturbances we must revert towards laminar motion which, by hypothesis, is unstable so that eventually new turbulence is generated, and so on. The distinction between the breakdown process and subsequent regeneration of an unstable system (associated with frictional dissipation of turbulent energy and radiative compensation of the heat transferred by turbulence) is logically convenient although such a clear-cut distinction is not observed in practice. What happens is that, precisely because the breakdown is irregular, the potentialities for the generation of turbulence are not immediately exhausted and regeneration begins while breakdown is still occurring. The result is that the actual mean motion is itself unstable. However the distinction is, at least in the present instance, quite trivial since the actual mean motion and the hypothetical laminar flow differ, in essence, only quantitatively. Now a state of motion is unstable if we can demonstrate the existence of *any* kind of unstable disturbance. Moreover on a uniform earth the only type of laminar motion we need consider is zonal motion satisfying the thermal wind equation, *i.e.*, steady baroclinic flow. But the writer has shown elsewhere (Eady 1949, pp. 36-46) that this flow is unstable. The disturbances which develop correspond to overturning of the air roughly in planes having half the slope of the broad-scale isentropic surfaces and result in the transfer of potentially warm air polewards and upwards side by side with equatorwards and downwards transfer of potentially cold air. (This type of instability is not to be confused with what is normally referred to as " dynamic instability " which involves overturning in a vertical plane.) If in place of the hypothetical laminar flow we commence with the actual (baroclinic) mean flow we obtain a similar result. (There is no " criterion " for the development of *this* kind of turbulence.)

Since the kind of turbulent motion which theory has shown ought to exist produces heat transfer of the kind inferred from observation we have some grounds for claiming to have explained in principle the fundamental cause of atmospheric motion. Complete quantitative explanation is not yet possible because of the rudimentary state of the theory of turbulence. Nevertheless the intimate relation between heat-transfer and the maintenance of turbulence is clear. We have already found it convenient to distinguish logically between the (adiabatic) generation of turbulence and the regeneration of instability by radiation. Considering the adiabatic part, the overturning process initially manifest as an unstable wave, we find that this occurs with the transformation of potential and internal energy into the kinetic energy of the disturbance. The potential and internal energies of the broad-scale distribution (which, as is well known, are closely related) are reduced precisely *because* potentially warm (and therefore less dense) air moves to higher levels and the mean centre of gravity of the atmosphere is depressed. A simultaneous poleward component of the motion of the warm air is necessary because continued displacement can take place (just as in ordinary convection) only if ascending air remains warmer than its " environment " and this implies motion at a smaller slope than that of the broad-scale isentropic surfaces. In this analysis we have ignored any contribution to turbulent energy from changes in zonal kinetic energy during the adiabatic process because calculation shows they must be quite small (of the order of the reciprocal of the broad-scale Richardson number). These changes are associated with changes in the thermal gradient consequent on the transfer of heat and with meridional displacements of air but since the initial state is restored later by radiation no meridional *circulation* occurs. The intimate relation between turbulence and heat-transfer is merely an expression of the fact that large-scale atmospheric turbulence is thermally driven. A similar relation holds for turbulence resulting from ordinary convection but *not* for turbulence resulting from shearing motion in a boundary layer or generated at a velocity discontinuity maintained mechanically. In these latter cases the intimate relation is between turbulence and momentum-transfer.

4. THE SIGNIFICANCE AND APPLICATION OF THE RESULTS OF PERTURBATION ANALYSIS

In view of the approximations made in the theory of instability of rotating baroclinic fluids and the rudimentary nature of the results one or two remarks may be apposite to avoid misunderstanding. Firstly, this theory was developed initially ignoring frictional forces whereas we have inferred that in the long run the kinetic energy destroyed by friction must exactly balance that generated in the atmospheric " engine." The point is that the theory, as developed so far, applies only to the initial stage of development of turbulent disturbances. In this stage, and only during this stage, the growth of kinetic energy considerably exceeds the frictional loss (as may be verified by rough calculations) and the modifications which would result from including frictional terms are quite small. This result is not really surprising for developments occur most rapidly (and become dominant) precisely in those regions where the excess of energy-generation over frictional loss is greatest and our approximations must be justified in these regions because of lack of uniformity. Of course a complete theory of the whole life-history of disturbances must take friction into account directly but the rudimentary theory of the initial stages is sufficient to prove that turbulence must develop. Secondly, the theory was developed on the assumption of adiabatic motion whereas in the long run radiation must balance dynamical heat transfer. Once again the approximations involved are justified during, and only during, the initial stages when the dynamic energy transfer considerably exceeds that due to differential radiation : they must be justified if we admit non-uniformity. Both in regard to friction and to radiation we find that the *logical* distinction made earlier between the overturning process and restoration of the *status quo* is not far from being an actual distinction so far as the initial development of an individual disturbance is concerned.

Thirdly, the theory was developed with approximations equivalent to the assumption that from *some* points of view the motion could be considered as incompressible. The aim was to eliminate certain elastic forces which would complicate but not affect the general nature of the results (they are important only for disturbances propagated at a speed comparable with that of sound) and to obtain a more tractable continuity equation, the modifications due to which were examined in the detailed analysis but not in the general energy theory. It should be added that the modifications in the general theory are now clear. The " equivalent incompressible " model used is valid for the actual, compressible atmosphere if we let " potential " energy in the model correspond to " potential-plus-internal " energy in the atmosphere. Then the two systems are *dynamically* equivalent (with the exception of a distortion of wave-form for very deep disturbances) and this is sufficient for the uses to which the theory is put. It is unprofitable to use the model for *thermodynamic* discussions because we have postulated a fluid more complicated than a perfect gas — one with a large coefficient of thermal expansion but zero compressibility : for this type of discussion we revert to a consideration of the actual atmosphere.

Fourthly, we have brushed aside the complications associated with condensation of water vapour. If the atmosphere were and remained everywhere saturated the modifications in the theory would be trivial. We need only replace the entropy of unsaturated air (differing insignificantly from that of dry air) by that of air plus water vapour plus condensed water (cloud), equivalent to replacing dry by wet adiabatics, and the general theory is modified only quantitatively. In practice however we very frequently find ascent of saturated air associated with descent of unsaturated air. It is important to note that this does not necessarily involve serious difficulties in the theory of the *initial* development of disturbances. The phenomenon of saturation is equivalent to a (very considerable) reduction in static stability (and consequently in Richardson number) so that developments tend to be concentrated in saturated regions and to be more rapid but they are of the same general type as those in unsaturated air. Complications arise later because air not originally saturated becomes so, and vice versa. We may expect that the effect on the structure of disturbances is important but there is no reason to infer any change in the fundamental nature of the overturning process. The latter is in many ways closely analogous to the vertical overturning which occurs during ordinary convection and we are aware that the type of convection which involves cumulus formation is fundamentally similar to " dry " convection. Further complications arise because developments in regions containing both saturated and unsaturated air generate velocity discontinuities on which we may expect turbulence of the mechanically driven type to develop. But such developments are clearly secondary and the transfer associated with this (smaller-scale) turbulence must be much less than that associated with the fundamental, thermally driven turbulence. From a thermodynamic point of view the important modification is that the heat of vaporisation to restore initial water vapour content must be added to that required to restore initial temperature when computing radiative restoration of the *status quo* and we must include latent heat with real heat in evaluating transport. Effectively the air at low levels is at a higher temperature than measured by the thermometer (not exceeding its equivalent temperature) and the distinction between real and latent heat appears in the immediate potentialities for development (while the air is unsaturated) rather than in the ultimate result.

Although the *ultimate* objective of our analysis is a complete quantitative explanation of atmospheric behaviour it is clear that owing to the rudimentary state of development of the theory we cannot at present hope to do more than make clear the basic fundamental principles. It is certainly encouraging to note that the dimensions and growth-rate of disturbances predicted by the theory of unstable waves on a uniform baroclinic current agree well enough in order of magnitude with observations and that there is also very frequently a fair amount of agreement as regards motion and structure but we can hardly expect anything like exact agreement nor should we be surprised if from time to time disturbances develop which appear to be of a different kind. As developed so far, theory refers only to an idealised system in which trains of disturbances develop independently in a regular manner. In practice disturbances develop from irregular systems irregularly and in short trains, frequently not exceeding a single wave-length. Whether or not it is theoretically convenient to regard disturbances as interacting they certainly appear to do so and it is only occasionally that we can identify a relatively " pure " wave-disturbance. This result is not surprising because theory *predicts* irregular development except for the *exactly* uniform initial condition postulated to give a tractable theory. It does however raise the question of what we mean by an individual disturbance. In actual irregularly-developing systems it may well be that we can assign no more than a rather hazy individuality to disturbances. If this is so it implies limitations to the present method especially as regards detailed bahaviour. But, in the first place, it is the virtue of the present approach that it makes readily intelligible the basic principles governing atmospheric behaviour and without this understanding we can hardly hope to develop a more refined technique. And, in the second place, the limitations are not necessarily so serious when it comes to evaluating large-scale long-term statistical behaviour : we have the example of statistical physics where a certain amount of haziness regarding the atomic constituents may not preclude the accurate evaluation of statistical properties. The point is that although individual disturbances may differ quite widely from the ideal model the basic driving forces exist, in a more or less distorted form, in the majority of them and must do so if the atmospheric engine is to continue working.

It is incorrect to argue that because atmospheric disturbances are most often " finite," *i.e.*, there does not exist a stage when we can see clearly an individual disturbance growing from small beginnings, we can learn nothing from the theory of small perturbations. The forces responsible for the instability of simple systems do not cease to operate when the disturbances become finite, otherwise a new steady flow would result, contrary to observation. All that is implied by the criticism is what has already been admitted, that the concept of an individual disturbance in practice is necessarily hazy and that we cannot hope to verify our theory from synoptic charts in more than a very rough manner. Independent verification is made more difficult by the fact that the concept of an individual disturbance is often even more hazy than it need be. Precisely because an adequate theoretical background has been lacking the idea has been allowed to grow up that the essential feature of the disturbance is a centre of low pressure on the surface chart. Now even rudimentary theory teaches us that the anticyclones (or ridges) play an equally important part and the term " disturbance " should apply to the complete structure. Moreover disturbances have a three-dimensional structure and in attempting to identify disturbances of a given type and the process which their development represents we cannot confine our attention to surface charts. If we merely follow surface " lows " we not only ignore independent evidence but run the risk of confusing a number of different things (changes of pressure associated with development on a larger scale, changes associated with change of latitude, changes associated with distortion of structure, *etc.*, as well as changes associated with true development). The relative (three-dimensional) motion of warm and cold air is often a better guide to what is happening because it is more directly related to the fundamental process which maintains atmospheric motion.

The dynamical part of the process envisaged consists essentially of a transformation of potential and internal energy into eddy kinetic energy and the type of disturbance which becomes dominant does so because it effects this transformation most rapidly. Even in the model, where development is supposed regular, the efficiency of transformation is not a maximum everywhere (*i.e.*, the motion is not everywhere in the plane corresponding to maximum energy-release) because of the constraints represented by all the equations of motion and boundary conditions. In the more complex (and more realistic) models we find regions where the reverse process is occurring : the principle of selection by maximum growth-rate applies to the *total* energy released. Although the details are not yet clear it is likely that the formation of jet-streams is an example of this reverse process. In the actual atmosphere, where development is irregular, the variety of ways in which the reverse process may occur in limited regions is increased. In particular, because large-scale disturbances are dispersive we may get energy concentrated in regions where it has not been released. That is why it was stated above that we should not be surprised to find disturbances originating in quite a different manner from those associated with the direct overturning process. But from a theoretical point of view all these complications are secondary in that they could not continue to occur without the main process, which alone is self-sufficient.

5. ANGULAR MOMENTUM TRANSFER BETWEEN LATITUDES

Let us now consider the problem of transfer of angular momentum between latitudes. As in the case of heat transfer we can infer from observations the actual transfer which we have to explain. Seasonal charts of surface isobars and surface winds indicate in each hemisphere belts of easterly winds in low latitudes and belts of westerlies in middle latitudes with smaller belts of easterlies in high latitudes. Although these are only average results there can hardly be any doubt that there is in each belt a mean torque due to friction at the earth's surface acting roughly in the opposite direction to the mean wind. Since friction is destroying westerly angular momentum in the west-wind belts and creating it in the east-wind belts, equivalent to a transfer of angular momentum from the west-wind to the east-wind belts since in the long run the total angular momentum of the atmosphere is unchanged, statistical equilibrium can be maintained only if some other process is continually transferring angular momentum in the opposite direction (see Fig. 2). This other process might be either meridional overturning or turbulent transfer associated with the large-scale turbulent motion whose existence has already been inferred. Now we have seen that meridional overturning cannot take place in the absence of turbulent transfer of angular momentum precisely because frictionless flow is associated with such a large angular momentum transfer. Hence the fundamental cause of angular momentum transfer and the consequent zonal currents must be turbulent transfer. What we have to show is that large-scale turbulence necessarily involves transfer of angular momentum. Having determined the turbulent transfer we shall then have to re-examine the question of meridional circulation.

The existence of turbulent motion allows us to infer directly, as a necessary consequence, *one* transfer property, that associated with the supply of energy to the turbulent disturbances, because the transfer must be in such a direction as to reduce the energy (kinetic or potential-plus-internal) of the mean motion. If atmospheric

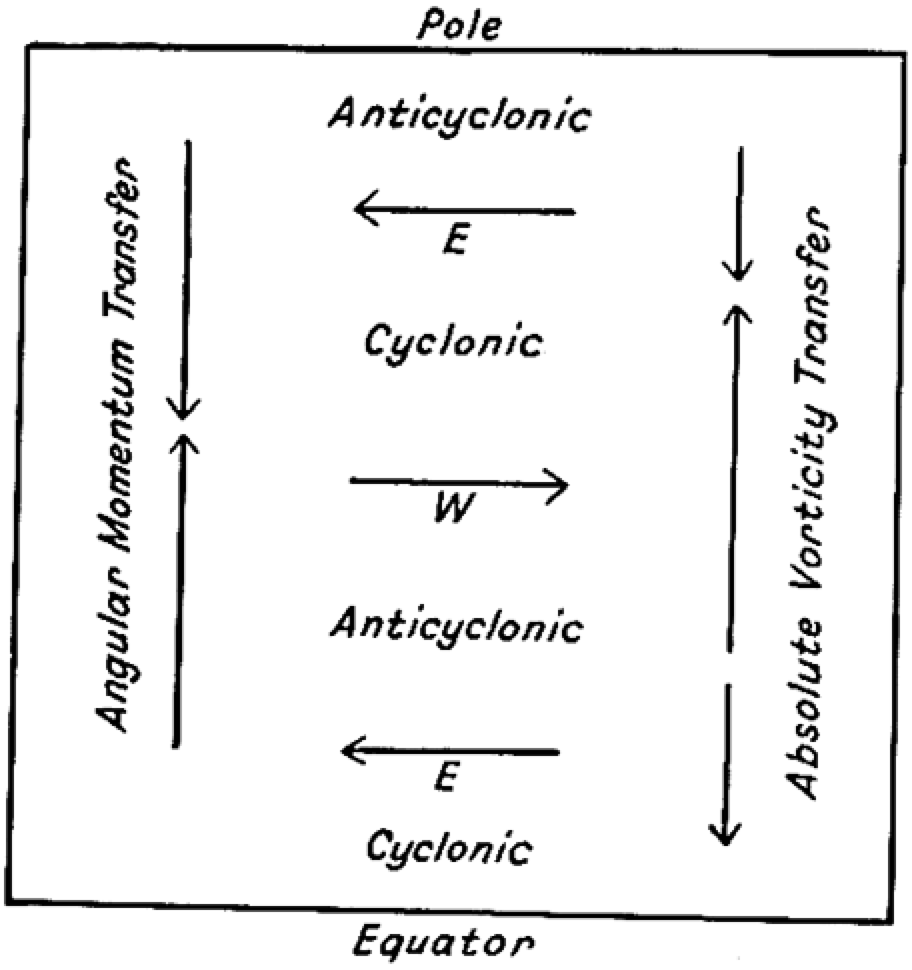

Figure 2. Angular momentum and absolute vorticity transfer between latitudes inferred from the observed zonal surface wind distribution. The figure represents a horizontal plane.

motion were mechanically driven this transfer property would refer to angular momentum and the turbulence would be such as to reduce zonal kinetic energy towards the minimum. It is easily demonstrated that this minimum corresponds to equality of mean angular velocity of the various zones. In order to maintain turbulence of this type we should have to have some other process acting which continually regenerated a state of inequality of mean angular velocity. It is true that such a process exists because a state of inequality is actually observed. (This is not quite the same thing as the inequality of angular velocity at the earth's surface, already referred to, because of the variation of wind with height but computations using upper air charts show that *mean* angular velocity is also unequally distributed.) But the only process which appears capable of accounting for this state of inequality is the *thermally* driven turbulent motion itself. In other words the inequality of mean angular velocity is not the source of turbulent energy but a result of turbulence. We have in fact an example of a *reverse* process (in a slightly more general sense than that in which the term was previously used) necessitated by the constraints imposed by the equations and boundary conditions of motion. The possibility of such a state of affairs is readily confirmed by computing the maximum growth-rate of hypothetical mechanically-driven disturbances and comparing them with thermally-driven disturbances, or, what is nearly the same thing, by comparing the available energies (*i.e.*, excess above the minimum produced by " mixing ") of kinetic and potential-plus-internal type. We find that the latter type of energy source very considerably exceeds the former, consistent with our supposition that it is the latter type which is the driver of atmospheric motion.

Our inference has been that the transfer of angular momentum is not *directly* connected with the maintenance of turbulence but is a secondary feature and we shall see that there is no general principle, as in the case of heat transfer, for determining its direction. In a sense the transfer of angular momentum is accidental since it is a by-product resulting from a particular set of constraints acting on developing disturbances. The mere existence of constraints is insufficient to determine the transfer or even its direction : we have to discover how these constraints act, *e.g.*, by considering their effect on individual disturbances. The analysis is made difficult by the fact that angular momentum, unlike entropy, is not even approximately conserved during motion. Within certain limits we can regard air particles as taking their entropy with them (*i.e.*, when the adiabatic approximation is valid) but it is only in very special cases that we can regard them, even very approximately, as taking their angular momentum with them : usually the torque associated with the pressure field is so large that the motion is nothing like that corresponding to conservation of angular momentum. We can speak of the transfer of angular momentum by disturbances because (and only because) the *total* angular momentum of the atmosphere is, in the long run, conserved but this is quite a different matter : the term " transfer " refers to the change produced in the mean flow pattern and does not necessarily, or even usually, imply simple bodily transport. The advantage of using conservative quantities when discussing transfer problems is so considerable that it is worth while examining the possibility of using some other quantity than angular momentum in our discussion. For example, if the vertical component of absolute vorticity were conserved during motion this would imply conservation of the total for the whole atmosphere and we could reformulate our problem as the determination of transfer of vorticity. Now in the very special case of purely meridional circulation angular momentum is conserved while in the very special case of purely horizontal barotropic flow vorticity is conserved. But in general, including the type of motion with which we are now concerned, *neither* quantity is conserved.

The transfer of any conservative quantity Q between two zones is obviously proportional to : $\int\int \rho V_y \cdot \Delta Q \, dx \, dz$ where ρ is the density, V_y the poleward velocity, x measures distance along a line of latitude, z distance vertically and ΔQ is the anomaly of Q (difference from mean value) per unit mass, the integration being over the complete circle of latitude and all heights. In other words the transfer is proportional to the correlation between ΔQ and V_y. Less obviously, the transfer of angular momentum, which is not conservative, is given by a formula of exactly the same type (Jeffreys 1926) *i.e.*, it is proportional to $\int\int \rho V_y \cdot r V_x \, dx \, dz$ where V_x is the velocity along and r the radius of the circle of latitude. (Another way of stating the result is that this formula measures the Reynolds stress.) The transfer is proportional to the correlation between V_x and V_y so that, knowing the observed transfer, what we have to explain is *why* V_x and V_y are correlated in a particular way. It is readily deduced from the observed zonal flow that we have to explain a positive correlation between V_x and V_y over most of the earth's surface but a negative correlation in high latitudes. It is interesting to note that the reality of this correlation has been shown by direct computation over a particular period, (Widger 1949).

6. HYPOTHESES OF CONSERVATION OF (A) VORTICITY, (B) ANGULAR MOMENTUM DURING TRANSFER

The form of the transfer formula does not distinguish between conservative and non-conservative quantities so it may not be immediately obvious why it is so much easier to deal with quantities of the former type. The fact is that when dealing with conservative quantities we can usually determine at least the direction of transfer at once. As an example consider hypothetical disturbances of barotropic flow in which the motion is supposed to remain horizontal. Then we know that the vertical component of absolute vorticity is a conservative quantity. If the mean flow pattern is similar to that observed on seasonal mean surface charts there is everywhere a strong gradient of mean absolute vorticity directed polewards. A particle which *has been* displaced polewards retains its absolute vorticity so that in the northern hemisphere it *must* have a negative vorticity anomaly. If the displacement is associated with a self-generating turbulent disturbance (initially an unstable wave) motion will continue in the same direction so that there is a negative correlation between poleward velocity and vorticity. Similarly, equatorward displacement is associated with positive vorticity anomaly and the turbulent motion as a whole must be associated with an equatorward transfer of absolute vorticity in the northern hemisphere. Taking into account the change in the sign of vorticity in the southern hemisphere we have the result that this kind of turbulence must give everywhere a southward transfer of absolute vorticity. Now from the observed flow pattern we have inferred the transfer of angular momentum needed to maintain it against surface friction and since, by hypothesis, total vorticity is conserved we may easily re-express our results in terms of the vorticity transfer needed to maintain the observed mean flow. We find that over most of the earth's surface (between very roughly 10° and 60° lat. in each hemisphere) the transfer must be northward, corresponding to the maintenance of the two major regions of vorticity anomaly, cyclonic anomaly centred near 60° lat. and anticyclonic anomaly centred near 30° lat. (see Fig. 2). This is directly contrary to the hypothesis that vorticity is conserved during motion. On the other hand the existence of minor centres of vorticity anomaly, anticyclonic in the polar regions, cyclonic near the equator, implies a southward transfer of absolute vorticity both in very high and in very low latitudes. In these extreme regions the hypothesis that absolute vorticity is approximately conserved *might* be valid. As a further example consider hypothetical disturbances in which flow is purely meridional so that angular momentum is a conservative quantity. Then since the mean flow shows a strong gradient of angular momentum which is everywhere directed equatorwards an argument similar to that used above shows that turbulent motion would be associated with a poleward transfer of angular momentum. This result is not *in itself* inconsistent with observation over most of the earth's surface but it is inconsistent with observation in high latitudes. Quite apart from other considerations relating to the possible mechanism of turbulence maintenance it is evident that neither the hypothesis of vorticity conservation nor the hypothesis of angular momentum conservation can account for the observed facts.

In passing we may note that with *conservative* quantities the transfer is always in such a direction as to destroy the gradient in the mean flow pattern (which, consequently, must be maintained by other means). Since in the dynamical theory we have treated entropy as a conservative quantity the result must apply to this particular case and it is easily verified that this is so. The apparent discrepancy, that entropy is transferred upwards against the gradient (for statically stable atmospheres) is easily removed. For when the static stability is positive there is always a horizontal component of the motion. Transfer takes place in a direction which makes a smaller angle with the horizontal than the isentropic surfaces and it is easily seen that its effect is to reduce the entropy gradient *in the direction of " mixing."* We may also note that this direction is different from that in which angular momentum is transferred. For angular momentum both the source and sink are on the earth's surface. Hence even if we can discover a process which transfers angular momentum in the free atmosphere we shall have to discover another process which brings the transferred momentum down to the earth's surface : in the free atmosphere the net transfer of angular momentum must be zero. We shall find that this downward transfer is effected by subsidiary quasi-steady meridional circulations which are in fact set up automatically. The possibility of such a solution (the subsidiary nature of the meridional circulation being a necessary condition) derives from the fact that meridional circulations transfer angular momentum vertically even more effectively than they transfer it horizontally.

7. THE CAUSE OF THE OBSERVED TRANSFER OF ANGULAR MOMENTUM

Since angular momentum is not conserved and we cannot determine the correlation between V_x and V_y directly from general considerations it appears that the only way in which this correlation may be determined theoretically is by computing the velocity field associated with individual disturbances. The computation is made difficult by the fact that, as might be expected from the consideration that the transfer is a *result* of the operation of constraints, we can only use models which reproduce fairly accurately the constraints (in particular the boundary conditions) actually obtaining in the atmosphere. (When we were studying entropy transfer this difficulty did not arise : the constraints could be varied within wide limits without affecting the direction of transfer.) It will be convenient to consider the effect of constraints as a combination of two separate effects.

The first effect, most noticeable in middle and low middle latitudes, is related to the curvature of the earth's surface in so far as this implies a variation of the Coriolis parameter with latitude. A " correction " to the most elementary theory which is most important for the larger (travelling long-wave) disturbances (Eady 1949, pp. 46-49) shows that besides the advective wave-velocity, corresponding to motion with the mean current in which the disturbance develops, and the purely imaginary wave-velocity corresponding to development there is an additional real wave velocity directed eastwards proportional to the square of the wavelength and the rate of change with latitude of the Coriolis parameter. This feature is similar to that exhibited by barotropic waves and has the same cause, namely the need to balance the divergence or convergence associated with north-south motion because of the variation of the Coriolis parameter. However the additional wave-velocity is smaller than for barotropic disturbances (especially for very long waves) because of the setting up of a kind of rocking motion (meridional motion alternating in direction along the wave). For present purposes we need not consider the details : the important point is that in their generally eastward progress disturbances are more retarded in low than in high latitudes. Now for a disturbance to maintain a constant shape it would have to move faster in low than in high latitudes because of the greater distance to be covered. Hence we should expect to find the axes of the ridges and troughs trailing away in a general NE.-SW. direction in the northern hemisphere and there is synoptic evidence that, on the average, this does happen. The argument may be made more precise. We find that theoretically maximum growth-rate corresponds to N.-S. troughs and ridges. Maximum *amplitude* must correspond to a later stage and therefore to disturbances in which the axes trail as described. Now it is easily verified, by substituting in the transfer formula, that disturbances the axes of whose troughs and ridges trail in this manner produce a poleward transfer of angular momentum (the geostrophic approximation may be applied). In the southern hemisphere we obtain the same final result. Hence this effect accounts for the observed direction of transfer of angular momentum over most of the earth's surface.

The second effect concerns behaviour in the extreme regions near the poles and near the equator. Both at the poles and at the equator the mean baroclinity is zero and a reasonably accurate model of the atmosphere would have maximum baroclinity (minimum Richardson number) in middle latitudes with decreased baroclinity towards the extremes. Analysis of such a model shows that disturbances should develop with maximum amplitude in middle latitudes and exponential decrease of amplitude towards the extremes. In the final stage we may expect to find that mixing extends into the extreme regions but rather as a process forced, through the operation of the constraints on motion, from outside. The energy transformations which drive the motion occur mainly in middle latitudes and in the extreme regions we may expect to find a quasi-barotropic driven turbulence. Now we know that in barotropic conditions (with horizontal motion) the vertical component of absolute vorticity is conserved and we have already considered the implications of this fact so far as transfer is concerned. We found that observed behaviour in the extreme regions was not inconsistent with the hypothesis that vorticity is conserved and the above argument suggests that there are theoretical reasons why such an hypothesis *ought* to give approximately correct results.

8. SURFACE FRICTION AND MERIDIONAL CIRCULATIONS

Since the amplitude of disturbances, both according to theory (Eady 1949, p. 48) and according to observation, increases on the average with height up to the tropopause, above which there is a rapid decrease corresponding to the greater static stability and smaller mean thermal gradient (larger Richardson number), most of the transfer of angular momentum (or vorticity) occurs in the upper troposphere. The sources and sinks are however, as we have noted, at ground level and to complete our analysis we have to show how the angular momentum transferred at high levels is brought down to ground level. A complete account would involve a discussion of the mechanism of small-scale turbulence in the friction (boundary) layer : for present purposes it will suffice to describe the main features. Although there are complications associated with small-scale convection this small-scale turbulence is fundamentally of the mechanically driven type, *i.e.*, the turbulent energy derives from the kinetic energy of the lowest layers. Associated with the development and maintenance of the turbulence there is, of course, a vertical transfer of momentum in such a direction as to destroy the kinetic energy of the boundary layer and were it not for the flow across the isobars towards low pressure the motion would eventually be destroyed. (In spite of complications similar to and perhaps more serious than those appearing on a large scale we can understand the maintenance of small-scale turbulence in terms of the continual growth of unstable disturbances, initially unstable waves of the Tollmien-Schlichting type or, especially over rough surfaces, of the Helmholtz-Rayleigh type developing on the associated velocity discontinuities.) The flow across the isobars in the boundary layer takes place automatically because the mean wind is less than geostrophic (as a *result* of turbulent mixing) so that so long as the mean flow is maintained the whole process will continue. Fundamentally the maintenance of the mean flow is by transfer of angular momentum between latitudes but we still have to show how the *whole column* of air contributes because we know (from the relation of isobars and wind in the free atmosphere) that small-scale turbulent transfer of angular momentum decreases considerably above the boundary layer. Now it follows immediately from the continuity equation that (at least in the long run) there must be a flow across the isobars above the boundary layer towards high pressure equal to the flow below towards low pressure. In other words the effect of surface friction is to generate meridional circulations. Logically we may consider the effect of such circulations apart from friction. Then we have conservation of angular momentum and it is easily seen that in the boundary layer the north-south flow is in such a direction that the transfer of angular momentum associated with meridional circulation compensates the loss associated with friction. Correspondingly, in the free atmosphere there is transfer of angular momentum in the opposite direction which in fact compensates the transfer of angular momentum due to large-scale turbulence. The net effect is that momentum brought in at high levels is transferred by the meridional circulation to the boundary layer for destruction by surface friction. In the long run the compensation is exact at each level because there is automatic adjustment of the mean motion until this is so (see Fig. 3).

The above argument simplifies the problem in so far as we have considered the effect of surface friction on the mean flow instead of the detailed large-scale turbulent flow but the more exact treatment yields only a quantitative difference in the final result. The greater the amplitude of large-scale turbulence the greater the effective coefficient of skin friction applicable to the mean flow : the direction of the cross isobaric flow is unaltered. (Not only does theory require this result but what actual computations have been made appear to verify it.) Hence in the zones of westerlies the lower cross-isobar current is directed polewards while in the zones of easterlies the lower cross-isobar current is directed equatorwards : in the free atmosphere the directions of the cross-isobar currents are reversed. Applying the continuity equation we find that there must be a general upward motion in the equatorial regions and in the low pressure regions near 60° latitude with a general

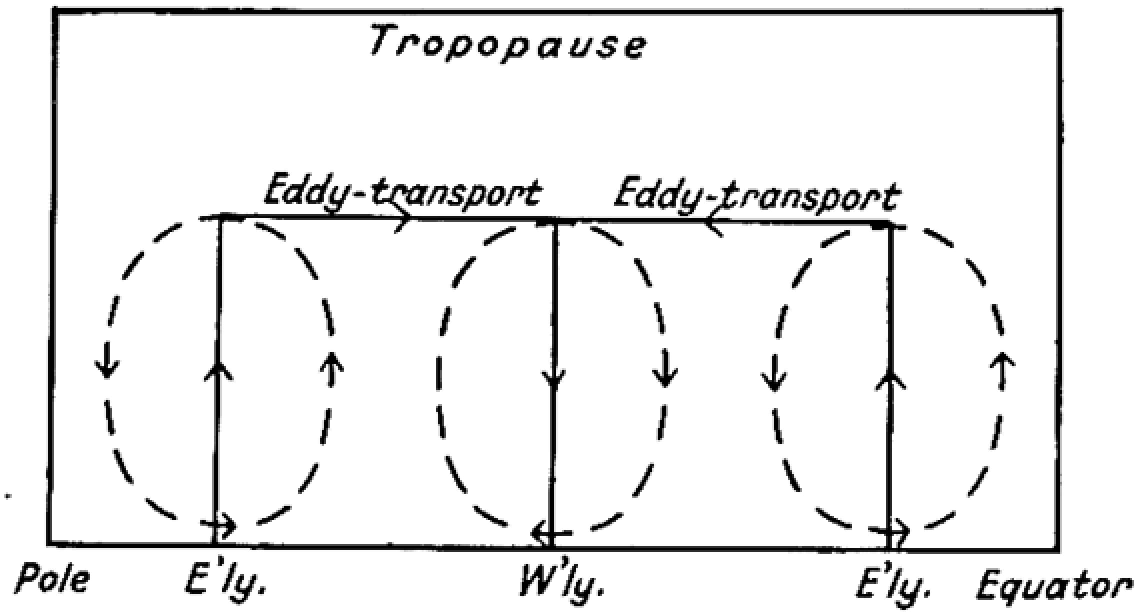

Figure 3. Transfer of angular momentum in the atmosphere. The dashed lines indicate frictionally driven meridional circulations which produce vertical transfer of angular momentum shown by full lines with arrows. The horizontal lines with arrows indicate transfer of angular momentum by large-scale turbulence.

downward motion in the region of the subtropical anticyclones and in the polar regions. In each hemisphere there are three meridional cells, two " direct " in low and high latitudes and one " reverse " in middle latitudes. It will be noted that *these* meridional circulations, in contrast to the hypothetical single direct-circulation considered earlier, are consistent with the observed zonal distribution of precipitation. Now both the direct and the reverse circulations are *driven*, they correspond to processes which absorb energy from outside, *i.e.*, from turbulent overturning. Only a fraction of the energy released by turbulent developments is utilised in this way and it is clear that if our theory is correct the rate of meridional overturning must be appreciably less than that of the hypothetical meridional circulation considered earlier, which was assumed to transfer heat at the same rate as that now attributed to turbulent transfer. Rough calculations based on an empirically determined coefficient of surface friction verify that this is so, the rate of circulation obtained being of the order of one-tenth that of the hypothetical circulation. The vertical velocities involved are now of a size with which observed deviations from radiation balance can cope, *e.g.*, the downward motion in the region of the subtropical anticyclones is so slow that the subsiding air is cooled by radiation fast enough to account for the observed lapse rate.

9. FURTHER CONSIDERATIONS

Our account of the mechanism of the " general circulation " of the atmosphere has been concerned only with the troposphere and the very lowest levels of the stratosphere. It is possible to consider events in this region independently of the rest of the atmosphere because the increase in (large-scale) Richardson number in the stratosphere reduces the amount of turbulent transfer and the tropopause acts in many ways like a lid. In the lowest layers of the stratosphere there is driven turbulent mixing as a result of the constraints expressed by continuity in the pressure field but at rather higher levels we should expect relative (though not complete) quiescence. At still higher levels the Richardson number probably again decreases and we should expect developments similar to, but not directly related to, those in the troposphere. An account of behaviour in these regions must await further empirical evidence. It seems unlikely that there is sufficient energy, either potential or kinetic, at very high levels for any *direct* major influence on tropospheric behaviour to be possible. It would however be erroneous to suppose that variations at very high levels (*i.e.*, well above the tropopause) can have no influence on weather. For example, an increase in stratospheric temperature, however slowly developed, would eventually lead to a lower tropopause : the boundary conditions applying to tropospheric developments would be altered, leading to disturbances of smaller size, a reduced rate of heat transfer and consequent modification in zonal characteristics. Most important of all there would result a change in the patterns associated with topography and the distribution of land and sea. Changes of phase (rather than changes in amplitude) of these patterns could be associated with very considerable changes in the climate of particular regions. A discussion of the manner in which these patterns are produced on a non-uniform earth is outside the scope of the present paper. It will suffice to point out that we can account for many features as the result of modifications in the zonal flow due to barriers (*e.g.*, mountain ranges like the Rockies and the ranges of E. Asia) and unevenness of frictional resistance (*e.g.*, owing to distribution of land and sea).

Finally, it should be pointed out that while the basic principles we have discussed are generally valid the application to zonal transfer is valid only for average behaviour (strictly that shown by annual mean charts, more roughly that shown by mean seasonal charts). Variations from mean behaviour are not only possible but are to be expected precisely because turbulent transfer is necessarily irregular.

The problem of the cause of the general circulation of the atmosphere may be regarded as the central problem of meteorology and a large part of meteorological literature bears directly or indirectly on it. Even to give a list of the principal works directly concerned with the problem would require greater acquaintance with the literature than the writer possesses. The following selection has been made therefore because these works are referred to in the text and not because they are, or are considered by the writer to be, the most important works. In addition to the references made earlier it should perhaps be emphasised that the *idea* of large-scale turbulence is not new — see for example Defant (1921). Also it may be noted that the principal features of the perturbation theory, on the results of which the present theory of the general circulation is based, are consistent with two independent investigations (Charney 1947, Fjortoft 1950).

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551.513.1

Comment on the global energy balance of the atmosphere

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Assuming viscosity and small-scale turbulence, the first principle of thermodynamics applied to the unit volume of air may be written in the following way (Van Mieghem 1949)

$$\rho q_e \equiv -\mathrm{div}(\mathbf{W}+\mathbf{W}_t) = \rho\frac{de}{dt} + p\,\mathrm{div}\,\mathbf{v} + \rho(\Delta_i - \Delta_m) + \rho L\frac{d\epsilon}{dt} \quad . \quad . \quad (1)$$

where ρ is the specific mass, e the internal energy per unit mass, p the pressure, $\mathbf{v}$ the air velocity, L the heat of vaporization, ϵ the specific humidity and t the time variable. Further $\mathbf{W}$ represents the radiative and conductive heat flux, $\mathbf{W}_t$ the turbulent heat flux, q_e the rate of external heating per unit mass, $p\,\mathrm{div}\,\mathbf{v}$ the work of expansion, Δ_i the work of the pressure and viscosity forces per unit mass along the path of the small-scale eddying motion, Δ_m Rayleigh's dissipation function, (amount of kinetic energy transformed by viscosity into heat per unit mass).

The rate of production of the small-scale turbulent kinetic energy per unit mass is given by $\Delta_i + \Delta_t$, where Δ_t is the amount of kinetic energy available per unit mass for conversion into kinetic energy of small-scale turbulence. When the small-scale eddying motion is stable, $\Delta_i < 0$ and Δ_i represents the amount of kinetic energy of small-scale turbulence converted into heat ; when $\Delta_i > 0$, the small-scale eddying motion is unstable and Δ_i represents the amount of heat converted per unit mass into kinetic energy of small-scale turbulence. The Richardson number h^2 may, in the general case, be defined as $h^2 \equiv |\Delta_i| : \Delta_t$. Now we must take into account the energy equation derived from the equations of motion and continuity, namely

$$\frac{\partial}{\partial t}\rho(k+\Phi) + \left[\rho(k+\Phi)v^\alpha + pv^\alpha - T^\alpha_\beta v^\beta\right]_{.\alpha} = p\,\mathrm{div}\,\mathbf{v} - \rho(\Delta_m + \Delta_t) \quad . \quad (2)$$

$$(\alpha, \beta = 1, 2, 3)$$

where $k \equiv \frac{1}{2} v^\alpha v_\alpha$ is the kinetic energy and Φ the potential energy per unit mass, v^α and v_α resp. the contravariant and covariant components of the velocity $\mathbf{v}$ in an arbitrary system of coordinates at rest with respect to the earth, and T^α_β the sum of the Navier-Stokes stresses P^α_β due to the viscosity and the Reynolds stresses R^α_β due to the small-scale turbulence, ($T^\alpha_\beta \equiv P^\alpha_\beta + R^\alpha_\beta$, $\rho\Delta_m \equiv P^\alpha_\beta v^\beta_\alpha > 0$, $\rho\Delta_t \equiv R^\alpha_\beta v^\beta_\alpha > 0$). The point to the left of the suffix α indicates a covariant derivative.

Substitution of (2) in (1) gives

$$\frac{\partial}{\partial t}\rho(e+\epsilon L+k+\Phi) + \left[\rho(h+\epsilon L+k+\Phi)v^\alpha - T^\alpha_\beta v^\beta\right]_{.\alpha} = \rho(q_e - \Delta_i - \Delta_t) + \rho\epsilon\frac{dL}{dt} \quad . \quad (3)$$

where h is the specific enthalpy of the air, ($\rho h = \rho e + p$). Integrating (3) over the entire volume V of the equatorial belt between latitudes ϕ and $-\phi$, and neglecting local variations because no long-run progressive change is possible in the *average* global energy within this belt, we finally obtain

$$\iint_S \{\rho(h+\epsilon L+k+\Phi)\,v_n-\mathbf{T}_n\cdot\mathbf{v}\}\,\delta S$$
$$=H-\left[\iint_\Sigma \mathbf{T}_N\cdot\mathbf{v}\,\delta\Sigma+\iiint_V \rho(\Delta_i+\Delta_t)\,\delta V\right]+\iiint_V \rho\epsilon\frac{dL}{dt}\,\delta V \quad . \qquad (4)$$

where v_n is the outward component of the velocity $\mathbf{v}$ across the vertical zonal boundaries S at latitudes ϕ and $-\phi$, $\mathbf{T}_n$ the frictional lateral stress exerted along S by the zonal belt V on the two remaining parts of the atmosphere, $\mathbf{T}_N$ the frictional stress exerted by the atmosphere on the earth along the earth's surface Σ between ϕ and $-\phi$, $H\equiv\iiint_V \rho q_e\,\delta V$, the total rate of heating of the air in the equatorial belt V. The terms in $\mathbf{T}_n$ and dL/dt are generally neglected and we shall accept these simplifications although they have not been hitherto justified numerically. Regarding the deletion of dL/dt it should be mentioned that evaporation and condensation do not occur at the same temperature. Moreover the terms in brackets in the second member of (4) are also usually deleted. But it should be emphasized that these terms are positive in the entire equatorial belt V; the first represents the dissipation of energy by ground friction and the second the total rate of production of small-scale kinetic energy in the entire volume V. We must keep them.

The introduction of the average value $\overline{F}$, along a latitude circle, of F and of the weighted mean $\widehat{F}=\overline{\rho F}:\bar{\rho}$ allows the evaluation of the energy flow in the first member of (4); one finds after some easy transformations

$$H=\iiint_V \rho(\Delta_i+\Delta_t)\,\delta V+\iint_\Sigma \mathbf{T}_N\cdot\mathbf{v}\,\delta\Sigma+\iint_S \bar{\rho}(\widehat{h}+L\widehat{\epsilon}+\widehat{k}+\Phi)\,\widehat{v}_y\,\delta S$$
$$+\iint_S [c_p\overline{\rho T''v_y''}+L\,\overline{\rho\epsilon''v_y''}+\overline{\rho v_x''v_y''}\,\widehat{v}_x]\,\delta S$$
$$+\iint_S\{\overline{\rho v_y''v_y''}\,\widehat{v}_y+\overline{\rho v_y''v_z''}\,\widehat{v}_z+\tfrac{1}{2}\overline{\rho[(v_x'')^2+(v_y'')^2+(v_z'')^2]\,v_y''}\}\,\delta S \quad . \qquad (4a)$$

where the symbols with two primes designate the *large-scale* fluctuations with respect to the weighted mean value along a parallel circle (v_y should be replaced by $-v_y$ at the southern border of the equatorial belt V); T, c_p, x, y and z have the usual meaning. The terms in the last line of (4a) are presumably of an order of magnitude less than the terms already neglected and consequently they may be deleted. If all these simplifications are acceptable the meridional energy flow is the resultant of

(a) a flow due to the mean meridional air flow $\overline{\rho v_y}$,

(b) a flow of sensible heat $c_p\overline{\rho T''v_y''}$,

(c) a flow of latent heat $L\,\overline{\rho\epsilon''v_y''}$,

(d) a flow due to the meridional flux $\overline{\rho v_x''v_y''}$ of relative linear zonal momentum;

(b), (c) and (d) are poleward fluxes and, according to Starr (1949), (b) and (c) are both of the order of magnitude $H/2$. From this, it follows that

$$\iint_S \bar{\rho}(\widehat{h} + L\widehat{\epsilon} + \widehat{k} + \Phi)\,\widehat{v}_y\,\delta S < 0$$

Now, if we assume in accordance with Rossby and Starr (1949) that $v_y = 0$ we arrive at an impossibility. On the other hand if we admit the existence of mean meridional circulations, $v_y \neq 0$, then v_y must be negative at latitude $+\phi$ and positive at latitude $-\phi$, at least where $\bar{\rho}(\widehat{h} + L\,\widehat{\epsilon} + \widehat{k} + \Phi)$ has the largest values, and so the mean meridional circulation $\overline{\rho v_y}$ at the borders S of the equatorial belt V would produce a resultant energy flux towards the equator. This result is in contradiction with the reasonable hypothesis of no long-run progressive change in the average global energy of the equatorial belt V, unless this energy flow is compensated in the equatorial belt by the production of kinetic energy of small-scale turbulence, by the dissipation of energy at the earth's surface and by an outflow of kinetic energy due to the meridional poleward flow of zonal momentum.

In order to avoid incompatibilities in the conclusions deduced from the global energy balance *all* the terms of the balance (3) or (4) should be carefully computed.

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The average planetary circulation in vertical meridian planes

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SUMMARY

The paper describes average conditions in the atmosphere from equator to pole to a height of 45 km, so far as they can be inferred from observations. It is deduced that there are two (or more) major vertical circulations, one above the other, running in opposite senses, in meridian planes. The nature of the circulation which should be set up by convection in an atmosphere unequally heated in different latitudes is then discussed theoretically. Dynamical reasons are suggested for the phenomenon of the stratosphere and for the height scale of the tropopause in middle latitudes and at the equator respectively.

1. DATA USED

Two or three years ago, in a report to the Meteorological Research Committee (Goldie 1947) I set out a cross section of the atmosphere from winter pole to summer pole up to a height of 45 km. The data used for winter were averages for November to January and for summer for May to July, for radio-sonde or ballon-sonde ascents for Abisko, England, India and Batavia, supplemented by air temperatures for England up to 45 km as derived from wartime experiments on the propagation of the sound of explosions (Murgatroyd 1947). The extrapolations required for other latitudes were made according to certain principles set out in the paper.

From the temperature field so obtained, cross sections showing pressure, potential temperature and density were computed.

Here I wish to show only two diagrams (one slightly modified) from the original paper and to comment on them.

2. General wind circulation

Fig. 1 shows isopleths of ${}^{\Delta p}/_{p/T}$ where p and T are pressure and temperature at the height and latitude in question and Δp is the difference of pressure from the pressure at the same level at the equator. Thus the isopleths show in effect horizontal pressure gradient at a given level divided by air density at the same level. The idea is to produce a vertical meridian section in which the horizontal spacing of the isopleths shall be proportional to the W.-E. or E.-W. component of the geostrophic wind, omitting only the latitude factor for which values are given in the original paper.

The diagram shows that there is in both summer and winter hemisphere, up to the 16 km level and excluding the shallow east wind in subtropical regions and doldrums, a west wind circulation around the pole with maximum wind speed just under the tropopause ; this polar vortex is a good deal stronger in the winter than in the summer hemisphere.

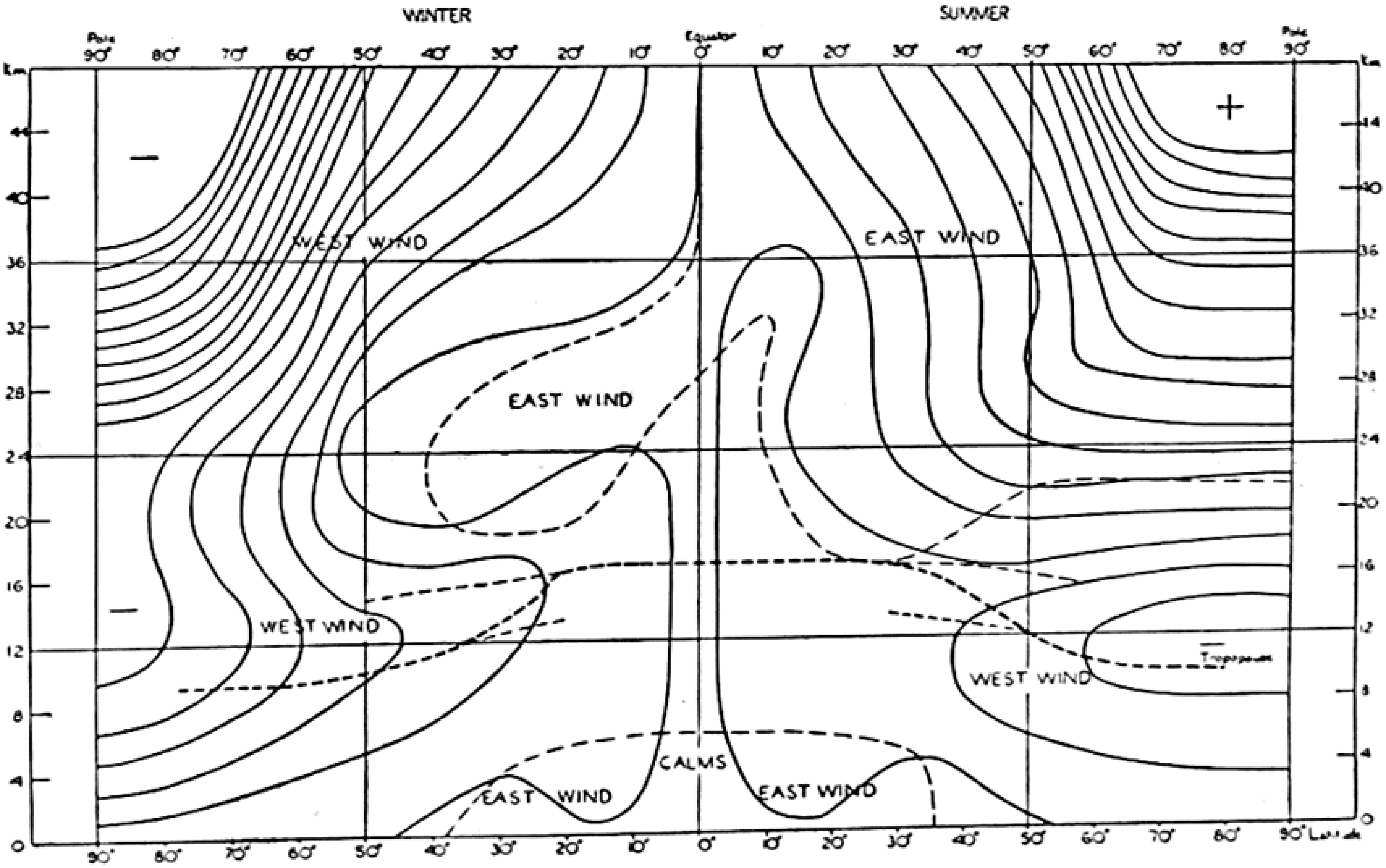

Figure 1. Distribution of ${}^{\Delta p}/_{p/T}$ and wind components perpendicular to meridian. (Northern summer. Broken lines show boundaries between W. and E. winds.)

In the winter hemisphere, from about 40° latitude up to the pole, it extends, with even greater horizontal speeds higher up, to at least 45 km. In the summer hemisphere, however, from about 22 km the wind changes over to an easterly (anticyclonic) circulation around the pole.

These circulations, for the latitude of England, have been confirmed both as to direction and mean speed for winter and summer by the wind measurements made at the 28 km level in the course of the Bruce gun experiments during the war (Murgatroyd 1949). The summer picture, with reference also to the change-over about the 22 km level has been again confirmed more recently by the radar wind measurements made up to 30 km in England during 1949 (Scrase 1949).

3. Circulation in meridian vertical planes

Fig. 2 shows isopleths of air density as percentage departures from density at the equator at the same level. The arrows on the diagram show supposed circulations in the vertical meridian plane. It will be seen that in each hemisphere there are three circulations, one above the other, running in alternate directions as if geared into one another. The evidence for the highest of these in the two hemispheres is not consistent and they must be regarded as doubtful. The two lower circulations in each hemisphere, however, *i.e.* below about 30 km, are such as would be promoted by the density field considered in relation to the isobaric (Bjerknes circulation theorem). The results of humidity observations in the

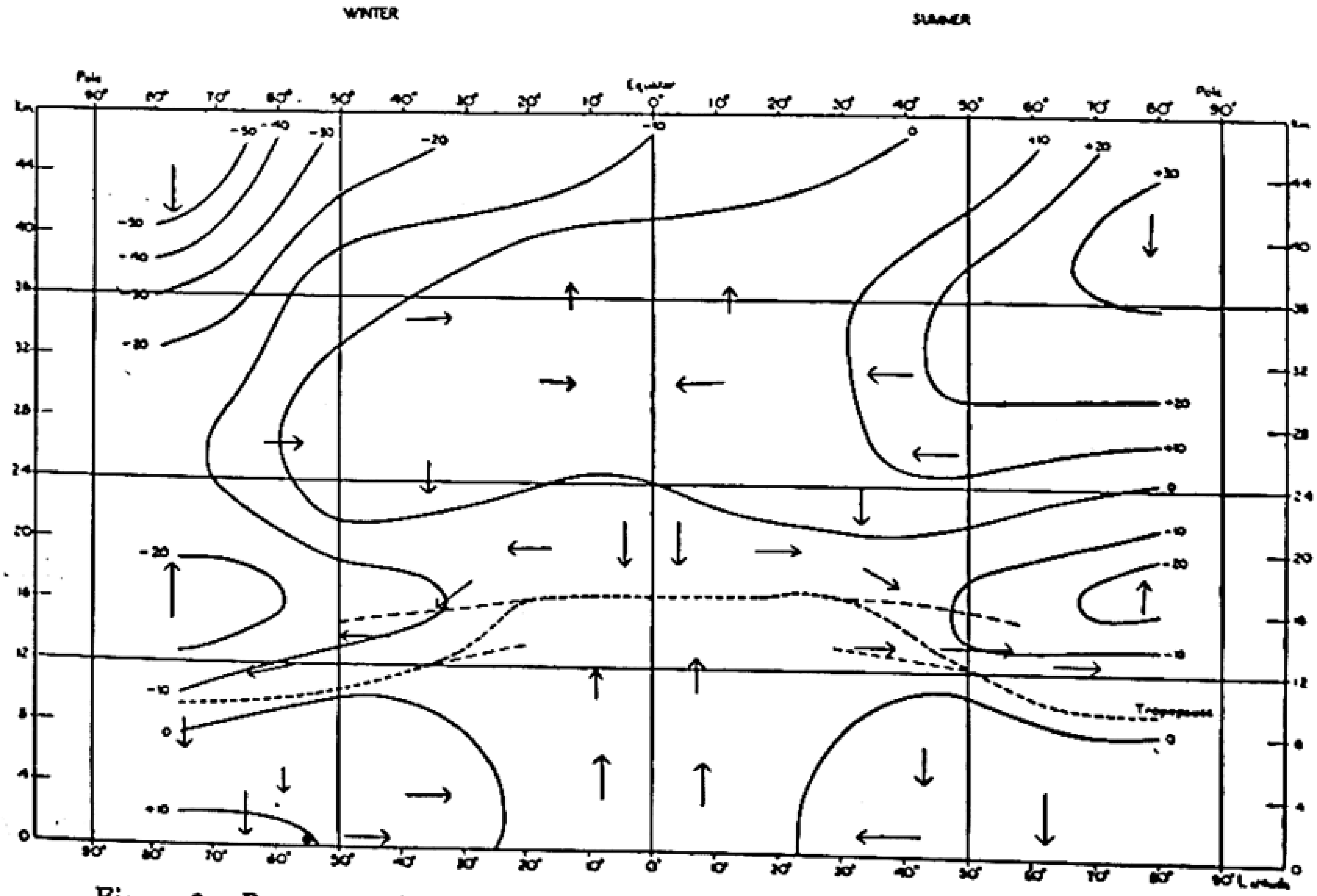

Figure 2. Percentage difference of air density from density at equator at same level. (Arrows show probable resultant circulation in meridian plane)

stratosphere in this country afford independent evidence in favour of such a system. And further, the meridional-vertical air movements harmonize broadly with the W.-E. or E.-W. movements shown in the zonal wind diagram, in the sense that where air is circulating from pole towards equator, the wind direction is east and vice versa. The meridional components are presumably relatively slow compared with the W.-E. or E.-W. component ; and it has to be remembered that the picture relates to seasonal (3-month) means.

This is the empirical picture, so far as it can be compiled from present day observations.

4. THE THEORETICAL PROBLEM

The question which arises is why there should be two (or more) vertical circulations one above another and not just one circulation in each hemisphere, upwards at the equator, downwards at the pole. The peculiarity that wind blowing along the surface of a liquid can result frictionally in a series of superposed vertical circulations was demonstrated, many years ago, by J. W. Sandstrom (1905). Also it was shown by Alf Sinding-Larsen (1902) that stratified circulations could be produced thermally in a test tube containing a heterogeneous salt solution, if the test tube is put in a glass of warm water. V. Bjerknes (1926) in *Solar Hydrodynamics* quotes both results and states that similar circulations should occur in the sun's atmosphere but does not take the matter further and indeed makes his computations on the solar atmosphere on the basis of one circulation, not twin or multiple circulations. The following is my attempt at a dynamical explanation of the twin (or multiple) circulations.

5. THE LEVEL OF MAXIMUM AIR MASS TRANSPORT IN THE PRIMARY CIRCULATION

Consider two columns of atmosphere, A and B, heated unequally ; in A, for two pressure levels p and $p+\delta p$ separated by a vertical distance δz we have

$$\delta z = -\frac{\delta p}{g\rho} \text{ or } -\frac{R\theta}{g} \cdot \frac{\delta p}{p}$$

where ρ, θ are the density and temperature at level p.

Similarly in B, for the same two pressure levels we have

$$\delta z' = -\frac{R\theta'}{g} \cdot \frac{\delta p}{p}$$

If A and B have the same pressure at ground level, then the total stretch of B as compared with A up to level p is

$$-\frac{R}{g}\int(\theta' - \theta)\frac{dp}{p}$$

the integration being taken from ground level p_0 up to level p.
The corresponding horizontal pressure difference Δp at a given height is given by

$$\Delta p = R\rho\int(\theta' - \theta)\frac{dp}{p}$$

If the numerical values of θ, θ' against height or pressure are known, the integral can be evaluated step by step. But since the lapse rate of temperature is much the same in the troposphere all over the globe we can get an approximation to the average conditions by taking $\theta' - \theta$ as constant. The integral then becomes

$$\Delta p = R\rho(\theta' - \theta)\log_e \frac{p_0}{p} \quad . \quad . \quad . \quad (1)$$

The value of p for which Δp is a maximum can be determined directly in two simple cases, (1) the atmosphere isothermal where $\rho \propto p$ and (2) the atmosphere in adiabatic equilibrium, where $p = k\rho^\gamma$. In the first case the maximum is reached when $p = p_0/e$ giving $p = 370$ mb which corresponds to a height of just under 8 km. In the second case the maximum is reached when $p = p_0/e^\gamma$ giving $p = 250$ mb or a height of about 10½ km. Conditions in the atmosphere should lie between these two cases and usually near to the second.

It will be noted of course that ρv the horizontal mass transport is proportional to Δp where there is geostrophic balance ; we have therefore shown that horizontal air mass transport tends to a maximum between about 8 and 10½ km in the primary circulation of convectional origin.

6. THE SECONDARY CIRCULATION

The secondary circulation (and any higher circulations), it seems to me, can only be explained as a consequence of the primary convective circulation. At any given high level (p) in the heated tropical region air is diverging northward and southward at a rate given by Eq. (1). Above that level the total removal of air is proportional to

$$\int R\rho(\theta' - \theta)\log_e \frac{p_0}{p}\, dz$$

the integration being taken from p to the top of the atmosphere. An air mass originally at the level in question would thus be subjected to an adiabatic reduction of temperature proportional to this integral multiplied by θ/p. Replacing dz in the integral by $dp/g\rho$ and taking the atmosphere as being arranged originally, approximately in adiabatic equilibrium*, integrating and multiplying by θ/p, we find the temperature reduction at the given level p to be proportional to

$$\frac{R}{g}(\theta' - \theta)\, p\, \frac{\gamma - 1}{\gamma}\left(1 + \log \frac{p_0}{p}\right) \quad . \quad . \quad . \quad (2)$$

This is a maximum when

$$\log \frac{p_0}{p} = \frac{1}{\gamma - 1}$$

or $p = \dfrac{p_0}{e(\gamma - 1)}$ or roughly $p_0/12$, corresponding to $p = 84{\cdot}5$ mb

or a height of 17·5 km.

Thus the consequence of the primary circulation is to lead to a maximum reduction of temperature (in the " warm " region) in that layer which originally was at a height of 17·5 km. It is noteworthy that this is about the height of the equatorial tropopause. The ratio of the temperature changes brought about in this way in a given time, at the 400 mb, the 84·5 mb and the 40 mb levels, for example, is 1·00 : 2·14 : 1·08.

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* The potential temperature in the tropics is in fact almost constant from 8 to 14 km or higher.

The result of the primary circulation therefore will be, in the " warm " region, to cause greater vertical stability and less mixing above 17·5 km but greater instability and more mixing below 17·5 km ; in the " cold " region, to cause greater stability below 17·5 km and greater instability above that level. Thus even if the atmosphere in both regions was originally stirred to adiabatic equilibrium at all heights (the initial condition most unfavourable towards explaining the final result) it could not continue in this condition if a primary convectional circulation set in from one region to the other. If in the " warm " region the heating is principally from the earth's surface, the convection of the heat upwards will soon become checked at the 17·5 km level and the temperature above that level will be determined in large measure by radiation. In the cold region, a condition of stability will spread downwards from this critical level and in these layers the temperature again will tend to be controlled by radiation rather than convection.

Though it is not practicable to express it in mathematical formulae it is presumably in this way that as between the two regions the conditions set up by the primary circulation sooner or later promote a vertical structure which reverses the slope of the isosteric surfaces in relation to the isobaric, beginning with the 17·5 km level, and produces the reversed secondary circulation with subsidence in the tropics above 17·5 km, in accordance with the empirical picture.

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Professor C. G. ROSSBY (*University of Chicago*) also contributed an opening paper on :

Comments on the structure of the zonal circulation

The manuscript is however not available for publication.

Discussion

DR. R. C. SUTCLIFFE (*Meteorological Office, London*) : Most of the contributions to this symposium on the general circulation have been concerned with important special aspects of the problem, vorticity and angular momentum considerations, the basic instability of zonal motion and the broad thermodynamical control. There has been little reference to the classical features of the general circulation and I would like therefore to outline an attitude to the problem arrived at in the attempt to integrate the fundamental work on radiation and dynamics with the experience afforded by daily studies of hemisphere synoptic charts. Although the ultimate object must be to derive everything quantitatively from physical principles it is also good to have at every stage a physical picture in front of us, a picture which seems to fit what we do know and which may indicate where quantitative results are still inadequate.

I am only allowed a few minutes and I shall attempt to sketch a general picture in order to ask whether the openers of this symposium have any quarrel with the outline as such. I hope also to have time to reach one interesting conclusion which I think is novel and important.

Starting with the differentially heated earth we assume that the primary tendency in the troposphere is towards high tropical temperatures and low polar temperatures. At both these extremes the differential heating with varying latitude is small, and the primary tendency is therefore to establish the strongest thermal gradients in middle latitudes.

The second principle of great importance is the well-known result that the couple about the earth's axis exerted by friction between earth and atmosphere vanishes in the mean as there is no secular change in the total angular momentum of the atmosphere. Thus east and west winds must, in this frictional sense, balance. We may perhaps start the argument, as does Dr. Eady, by imagining a state of universal surface stagnation with a zonal flow at higher levels given by the thermal wind relation. Now, synoptic evidence shows that such a state when approximated in nature breaks downs, indicating dynamical instability of zonal flow, and much theoretical work is now available to confirm this experience. Dr. Eady has made notable contributions to this dynamical problem and my own theoretical work has led to general results which seem to explain many synoptic developments tolerably well. It is, I think, generally agreed that the cyclonic and anticyclonic developments of middle latitudes are the main manifestation of this lack of stability of zonal flow. Such incomplete theory as there is indicates that the sort of circulations which we observe are a necessary consequence of the thermal gradients and lapse rates, that the scale factor is dynamically determined and that developments should be most vigorous where the thermal gradients are strongest — again confirming experience.

I therefore postulate that the immediate consequence of the primary state of zonal flow would be a highly disturbed flow in middle latitudes with cyclones and ridges such as we observe : cyclonic development would predominate owing to the greater instability of the cyclonic perturbation with its internal supply of energy. It is also easily shown theoretically, on the principle of contour steering, and is well known as a forecasting principle, that the cyclonic disturbance should tend to move polewards as it develops and to stagnate on the poleward side of the main thermal belt while the anticyclonic disturbance should tend equatorwards to stagnate on the warm side of this belt. Thus we may infer from dynamical-synoptic principles that the statistical result should be a disturbed middle latitude belt of prevailing westerlies.

To proceed further it is necessary to have a theoretical demonstration of the latitudinal scale factor of the perturbations but, until the theorists can provide this, it is fair to take synoptic evidence which shows that the cells appearing at surface levels are roughly circular with a north-south " wave length " similar to the east-west value, in the order of 60° of latitude for the larger systems, say 6,000 km. If the mathematicians can show that the major perturbations must be of such a size, and as I understand them they seem to be well on the way, we have the important result that the earth is barely large enough to contain more than one belt of such disturbances and the main pattern of the general circulation is set immediately by the synoptic-dynamical principles.

Time does not allow me to comment on the equatorial zone but if the synoptic scale factor of the disturbed middle latitudes sets the pattern of the sub-tropical high pressure belt it is at least not surprising that there should be room on the earth for one, and generally not more than one, further " cyclonic " cell between the hemispheres.

It is therefore postulated that the general circulation pattern is determined dynamically by the scale of the synoptic-dynamical systems which can develop most readily and dominate the flow on this earth. And in this connexion the mathematicians, or the less rigorous speculators on the mechanism of the general circulation, must be careful not to prove too much. The classical general circulation model, with a simple equatorial zone, sub-tropical highs, disturbed westerlies and mainly anticyclonic polar regions, may be common but is far from permanent. Quite frequently the circulation breaks down into a very different régime which may persist for days or even weeks and a double system of disturbed westerlies, separated by a second anticyclonic belt, is not uncommon in the northern hemisphere. It appears that the classical model is not always the preferred mode of the general circulation.

But I now wish to pass on to an important thermodynamical feature which may be of general interest. An anticyclone in development is a subsidence region and a quasi-permanent anticyclone maintains continuous frictional subsidence. The result, except over a strongly heated ground, is stability in the lower layers and the vertical eddy flux of heat is downwards to the cooler surface of the earth (or in some cases to a low cloud layer which acts as the radiating surface). Radiation theory, of which we heard in our first symposium, shows that the troposphere is cooling also by this mechanism and the inference is that *anticyclonic regions are important atmospheric heat sinks.* Rossby has, I believe, somewhere remarked that the atmosphere has a way of controlling its own sources and sinks of heat and here we have the mechanism. It is of course thermodynamically necessary that a persistent, quasi-permanent, subsiding region should be a heat sink in order to permit the air to subside through the isentropic surfaces and the theory of heat flux by turbulence and radiation permits of this.

In considering the thermodynamics of the atmospheric circulation in general we are likely to go entirely wrong if we start with the gross simplification of a low-level heat source near the equator and a high-level heat sink mainly in high latitudes: I see absolutely no basis for this. The atmosphere is primarily heated from below and cooled above and if a steady state of radiative flux were established everywhere it is likely that, broadly speaking, the heat source would be present at low levels in all latitudes. But the dynamical break-down sets the pattern of the circulation so that the anticyclonic regions become heat sinks throughout the troposphere and the cyclonic regions heat sources (partly due to ordinary eddy flux in unstable air but, in our atmosphere, largely due to the liberation of latent heat of water vapour).

Thus when discussing the general circulation, we have no adequate *a priori* knowledge of the distribution of heat sources and sinks nor, indeed, of the number of zonal belts of easterlies, westerlies or maxima of thermal gradient. Differential solar heating presumably defines the broadest features of the temperature level but we cannot infer the resulting thermal gradients nor the heat sources and sinks until we know the circulation model.

Dr. Nyberg : Professor Van Mieghem in his paper discussed the northward flux of sensible heat. A computation of this flux has been made for a single station by Priestley (*Quart. J. R. Met. Soc.*, 1949, **75**, p. 28) and I have made a computation of the heat flux on a synoptic basis using hemispherical charts analysed from Alaska to West Siberia. Choosing limiting points, having the same geopotential, in the western and eastern parts of a latitude circle and assuming geostrophic winds there will be no net transport of mass across this part of the latitude circle. The product VT (V being southerly winds and T the temperature) has been computed for 10° strips of the latitude circle and the totals of VT have then been formed. The transport for 60°N. and the 850 mb level has been plotted on a diagram as a function of time and values of the mean zonal wind between 55°N. and 65°N. have also been plotted on the same diagram. One finds (i) the correlation between transport of heat and zonal flow is very high, and in contradiction to earlier assumptions, positive ; (ii) there is a lag of approximately one day between the curves, an increase of heat transport being followed by an increase of the zonal flow.

Dr. Bleeker : I have the impression that Fortune smiled on me when Dr. Rossby was showing his mean cross section. That over India was certainly related to a zone of strong differential heating (North Indian plain — Himalayan Mountains and Plateau of Tibet). Also the cross section of Eastern Asia went through a zone of strong differential heating. I have always had difficulty in forming a picture of Dr. Rossby's theoretical considerations on the formation of the jet stream that was directly interpretable and which was intuitively physically understandable and I know that other synopticians have analogous difficulties. Could Dr. Rossby explain more than one jet stream at different latitudes in his theory ? I am glad to hear that Dr. Petterssen has also given differential heating such an important place in his discussion on the general circulation. I am very much intrigued by the zone of cyclogenesis on the east coast of the United States and think that this also is a result of front-formation along a zone of differential heating.

Lieut. Cdr. D. F. Rex, U.S.N. : As one member of a small group working under Professor Rossby's supervision in Stockholm on the problem of " dynamic climatology," I have undertaken, during the past year, a study of blocking action in the middle troposphere and its effect upon regional climate.

It will not be possible to discuss this work in any detail in the time available, but I should like to show one example of strong blocking (one of five seasonally and geographically distributed cases which have been analysed in detail) and to comment upon its climatological significance. The example chosen (not reproduced), a summer case, developed from a strong ridge situated over the 15th meridian west at the 500-mb level on 15 June 1949 and ended with the disappearance of the resulting warm anticyclone over Finnish Lappland early in July. I should like to call attention to the strongly marked discontinuity in the westerly flow which persists throughout the blocking period and which marks the point of sharp transition from essentially zonal flow upstream to a predominantly meridional type downstream, this downstream motion being characterized by a train of successive large-scale anticyclonic and cyclonic turbulence cells. A computation of time and space mean-velocity profiles shows a single relatively strong and narrow jet (westerly) located at 51°N. in the upstream sector which splits into two branches, located at 73°N. and 40°N. downstream, and between which an easterly flow develops. It appears difficult to account for a persistent flow pattern of this type as the result of the development of impressed perturbations as long waves. A theory recently developed by Prof. Rossby (published in China — 1949) which concerns the development in quasi-horizontal planes of " atmospheric jumps " predicts that in westerly streams the formation of such a jump *may* occur whenever the ratio $u/\beta a^2$ exceeds a certain critical value (u is zonal speed and a is stream width). It seems likely that blocking situations arise therefore from relatively narrow high-speed jet streams which are acted upon by a sufficiently intense external impulse, usually in the form of a deep cyclonic vortex adjacent to the stream's northern boundary — all cases which have been analysed support this view.

Finally I should like to describe some climatological influences of a typical blocking situation over north-western Europe. Considering first the surface-temperature anomalies produced during January 1947 (see Fig. 1), we observe a belt of positive temperature anomalies (+6°F) extending across the North Atlantic over Spitsbergen and Scandinavia into parts of North-west Russia. At the same time a tongue of below-normal temperatures (—6°F) extends from western Russia across Central and Southern Europe to the French Atlantic coast. The similarity between this pattern and that obtained by Prof. Willett*, showing the change in average winter temperatures which has occurred between the third and fourth decades of this century, is striking. The changes produced by blocking in the circulation at 500 mb are characterized by a sharp increase in meridional flow components over the eastern Atlantic and over continental Europe between approximately 45°N. and 65°N. At the same time we observe, over Europe, a sharp increase in the zonal westerlies above 70°N. and below 45°N. with a marked decrease over the upper-mid-latitudes. This agrees well with the results recently published by Dr. Petterssen showing the changes which have occurred in the winter surface circulation over the same region between the two periods 1900-19 and 1920-39.

I should like to repeat Dr. Lysgaard's earlier statement concerning the importance of blocking action in the recent climatic change and wish specifically to suggest the more frequent occurrence and longer persistence of blocking situations over the European area as a possible, and to my mind, probable factor of importance in producing the climatic trend observed during the past thirty years.

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* See p. 195 of this issue.

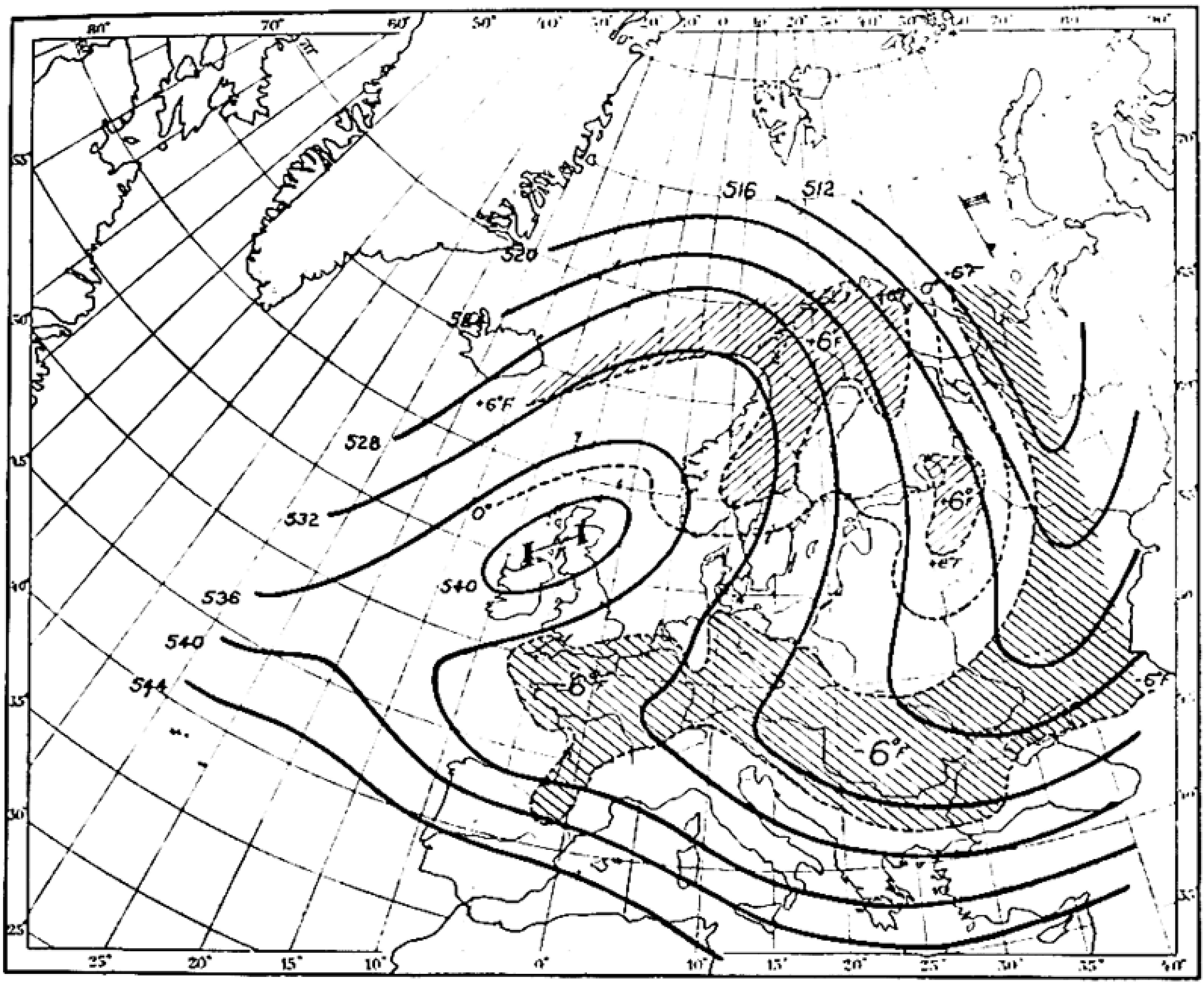

Figure 1. Mean 0600 GCT 500 mb chart. Heights in dyn. decametres. Surface mean-temperature anomaly 1901-1930 normal. 16-31 January 1947.

———————— Heights

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Mr. D. G. Harley (*communicated*) : I understood Dr. Sutcliffe to describe the sequence of events in the development of a warm-sector depression as

(1) distortion of the westerly flow at low levels which carries the warm air north and the cold air south ;

(2) the resultant distortion of the thickness pattern which alters the upper winds to a SW.-NE. direction.

Surely this is the wrong order of events, though it is commonly so given ? The pressure field at any level, controlling the winds at that level, depends only on the mass-distribution above that level, and cannot be directly affected by changes in the density-distribution at lower levels ; while changes in momentum at lower levels can only be transmitted upwards by vertical motion. On the other hand, pressure changes at upper levels are projected directly on lower levels, though the effects may be modified by vertical motion. This suggests that the changes in the upper wind field, and hence the fate of the surface disturbance, depend primarily on the general circulation.

I think that contour charts, otherwise most useful, encourage a faulty process of thought, in that it is easy to suppose, carelessly, that the height of the constant-pressure surface depends on the air below that surface, whereas one would not dream of attributing the pressure at a given height to anything other than the air above. Also, the 'thermal wind' is usually considered as modifying the surface gradient, and is quoted as the reason for strong upper winds. Surely it is nearer the truth to regard it as modifying the upper winds to produce the surface winds?

MR. W. A. DWYER (*Australian Meteorological Service*): Meteorological investigations into southern hemisphere meteorology are now being sponsored by several countries, especially by those countries such as South Africa, New Zealand and Australia, located in the southern hemisphere. It is likely that these investigations will throw new light upon the circulation of the atmosphere in the southern hemisphere. The recent work of Loewe and Radok of Melbourne University, on the mean temperature distributions and the mean zonal wind components along long. 150°E., is quoted as an example. It appears from this work that there is strong evidence of the existence of 'jet streams' across certain latitudes over the Australian continent. (See a forthcoming paper in the *Journal of Meteorology*.) Observational and synoptic evidence of 'jet stream' conditions over Australia was also increasing in the Commonwealth Meteorological Service.

MR. L. C. W. BONACINA (*communicated*): If by the rather vague term "general circulation" is meant the actual detailed pattern of the circulation over the entire globe at any moment of time I do not think we can ignore the probability that the geographical differentiation of the earth's surface plays a very important part in determining that pattern and does much to transform one type of weather into another.

On a uniform, or nearly uniform globe the thermal slope between the equator and the poles would, assisted by geostrophic force, give rise to the broad planetary belts of wind and pressure that we know, and eddies of some kind or other would no doubt produce simple daily weather changes, but I do not think accentuated weather types would be possible.

The process I have in mind is something like this: a great continental land mass, or a vast ice sheet, may in winter be regarded as always biding a favourable phase of the general circulation to permit it to cool down rapidly by radiation and so induce high pressure over it through intense cold. Should this effect be dominating then the neighbouring circulation will have to accommodate itself thereto besides having to respond to influences elsewhere. In this way an extremely complicated chain of reactions will be set up through a wide area, producing that multiplicity of depressions, anticyclones, fronts and air masses which constitute the actual surface circulation.

Conversely in summer a great land mass may be thought of as always awaiting a phase in the general circulation favourable to strong sunshine which will permit it to heat up intensely and so engender low surface pressure with deformation of the air currents all around and adaptations far and wide.

Ever since some twenty-five years ago circumstances arose which forced me to think out all the implications of the theory of continentality I have held the view that weather results from the interaction of the surface features with the upper air circulation, and this view is implicit in much that I have written on climatological subjects since. If it is the general thermal gradient from the equator to the poles which initiates or maintains the general circulation it must be the innumerable local and regional thermal effects due to geographical differentiation which complicate and develop the pattern, always giving fresh tilts that result in accentuated phases of weather.

I must try and formulate a summary of the proposition as follows : The differential regional and local thermal effects, according to season of the year, due to land, water, ice or snow sheets, ocean currents, deserts, mountains, forests, *etc.*, modify the surface systems of wind and pressure at a particular time in a manner which depends on the phase of the circulation at that time ; or more briefly weather is a compromise between its surface factors of the circulation and the hydrodynamic factors.

Dr. Petterssen (*in reply*) : If I have understood the foregoing discussion correctly, Prof. Bleeker takes exception to the dynamic approach to the general circulation and maintains that local differential heating associated with the distribution of land and sea, *etc.*, is all-important. While this may be true of local and stationary circulation systems, it appears difficult, on this basis, to account for the zonal pattern of the general circulation. The pronounced zonal belt of cyclogenesis and anticyclogenesis across the Pacific Ocean (*e.g.*, Figs. 14 and 18), where local temperature contrasts are almost absent, seems to indicate that the general circulation would be *essentially* the same if the earth were uniformly covered by water. A belt of dynamic instability with travelling cyclones and anticyclones and a jet stream at high levels would still be found in middle latitudes. The irregularities of this zonal belt, as actually observed, represent, I believe, the local influences which may be either of thermal or of topographic origin.

CENTENARY SYMPOSIUM
ON
CLIMATIC CHANGE

Held at Oxford on the afternoon of 30 March 1950

Chairman : PROFESSOR GORDON MANLEY

551.583.3

On the value of palaeoclimatological evidence

By KNUT FAEGRI
Bergen Museum

Palaeoclimatological evidence is indirect ; before the era of meteorological registration the only available evidence of the character of the climate is that furnished by biological and geological phenomena, the developments of which are influenced by climate. It is in most cases extremely difficult to express this evidence in terms of meteorological units without resorting to auxiliary hypotheses, which are all the more dangerous as they are frequently implicit. A very interesting example of this is given by a recent, extremely important paper by Buedel (1949). His starting point is a set of two maps of the present snow-line and that of the last glaciation*. By means of these maps and by a very interesting and comprehensive analysis of the vegetation regions of Europe during the Ice Age, Buedel concludes that the weather systems were in principle the same then as they are now — which is, I think, a very likely conclusion. But his use of the snow-limit depression data for the calculation of isotherms presupposes that the climate of Europe was then of the same type as it is to-day.

Gunnar Andersson (1902) published a very beautiful map of the present and fossil distribution of the hazel in Sweden. The limit of the distribution area during the post-glacial warm period very nearly coincides with the present July isotherm of 9·5°C, whereas the present limit coincides with the 12°C isotherm. Consequently Andersson concluded that the July temperature had fallen 2·5°C since the optimum period of the Post-Glacial.

Gunnar Samuelsson (1916) again took up the same problem and showed that the summer temperature along the present northern limit of hazel was far from uniform, but that a lower summer temperature could be compensated for by a longer vegetation period. The climatic demands of the hazel along its northern limit are thus expressed by an equation

$$v = f(t)$$

where v is the duration of the vegetation period and t is the temperature of the summer. If v is expressed in weeks, Samuelsson found that the demands of the hazel could with very close approximation be described by

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* To what extent our knowledge permits the construction of such detailed maps may be questioned, but for the present I shall not take up that point for discussion.

$$v = 25{\cdot}32 - 0{\cdot}47t$$

This means that our palaeoclimatological thermometer does not give us a single figure for an answer, but it gives us a series of temperatures, each corresponding to a given duration of the vegetation period.

However, Samuelsson's equation is only part of the answer — it is obvious that the line described by the function between $v = 0$ and $v = 52$ gives us a much too wide range of temperatures. Iversen (1944) took up the problem once more, and by a similar technique (Fig. 1) he showed that the climatic demands of a species near its temperature limit is described by two limiting temperatures, one for the cold season, and one for the warm. If winter temperature is lower than a certain value, t_1, the species is excluded, however warm the summer, and if the summer temperature is lower than another value, t_2, the species is also excluded, however mild the winter. If, however, these two demands are satisfied, Iversen finds a regression similar to Samuelsson's, but it is certainly not linear.

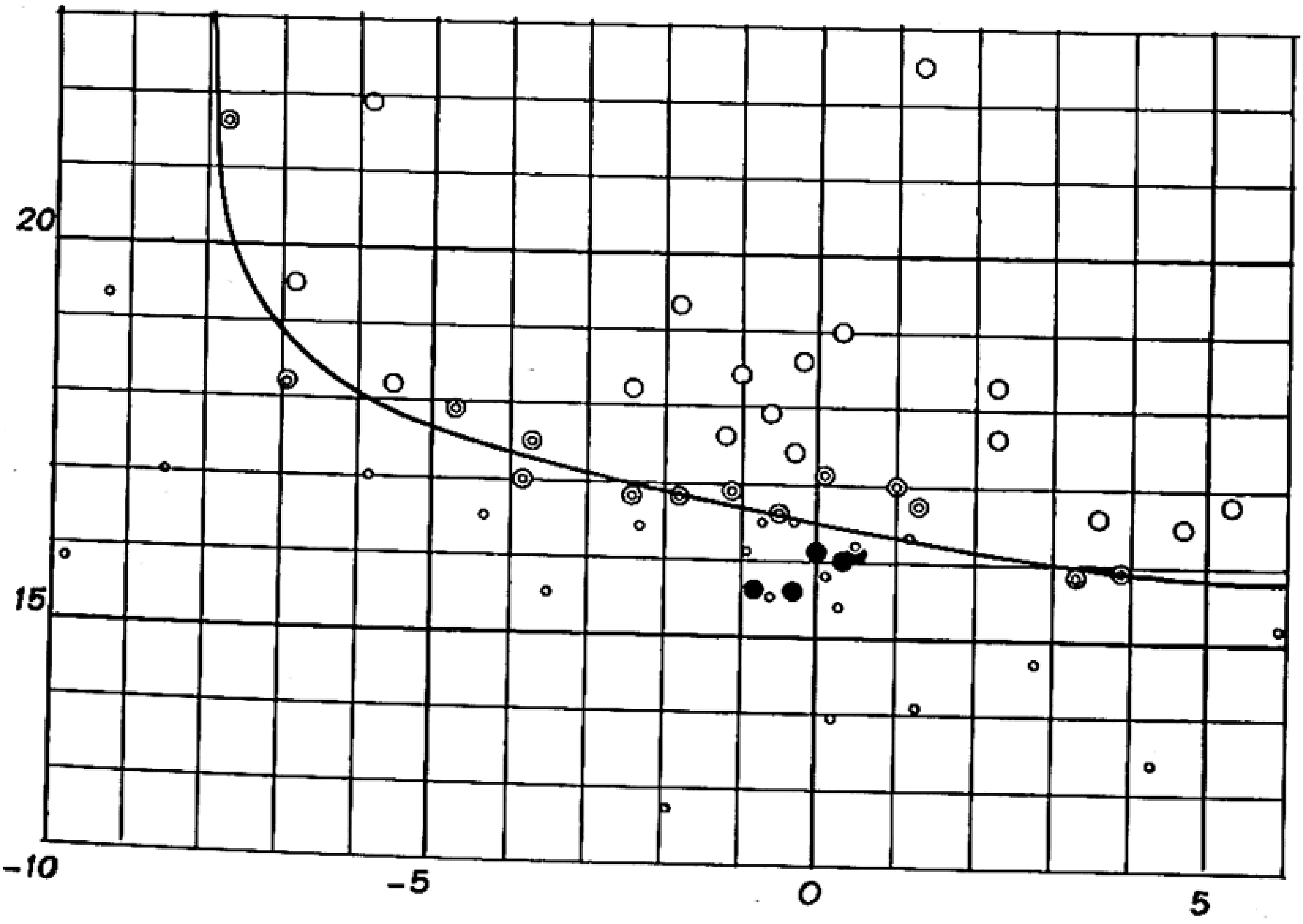

Figure 1. Iversen's diagram illustrating the climatic demands of the mistletoe. Ordinate: mean temperature of warmest month, abscissa: mean temperature of coldest month. Rings indicate the position of meteorological stations in the coordinate system. Small rings: stations outside the area of distribution of the species; big rings: stations inside that area. Double rings: stations on the present distribution limit. The dots indicate some finds of fossil mistletoe outside the present area. The limiting temperatures seem to be *ca.* 8° in winter and 15·5° in summer. Samuelsson's rectilinear regression evidently represents the part of the curve between *ca.* –5° and +5°. Redrawn after Iversen (1944).

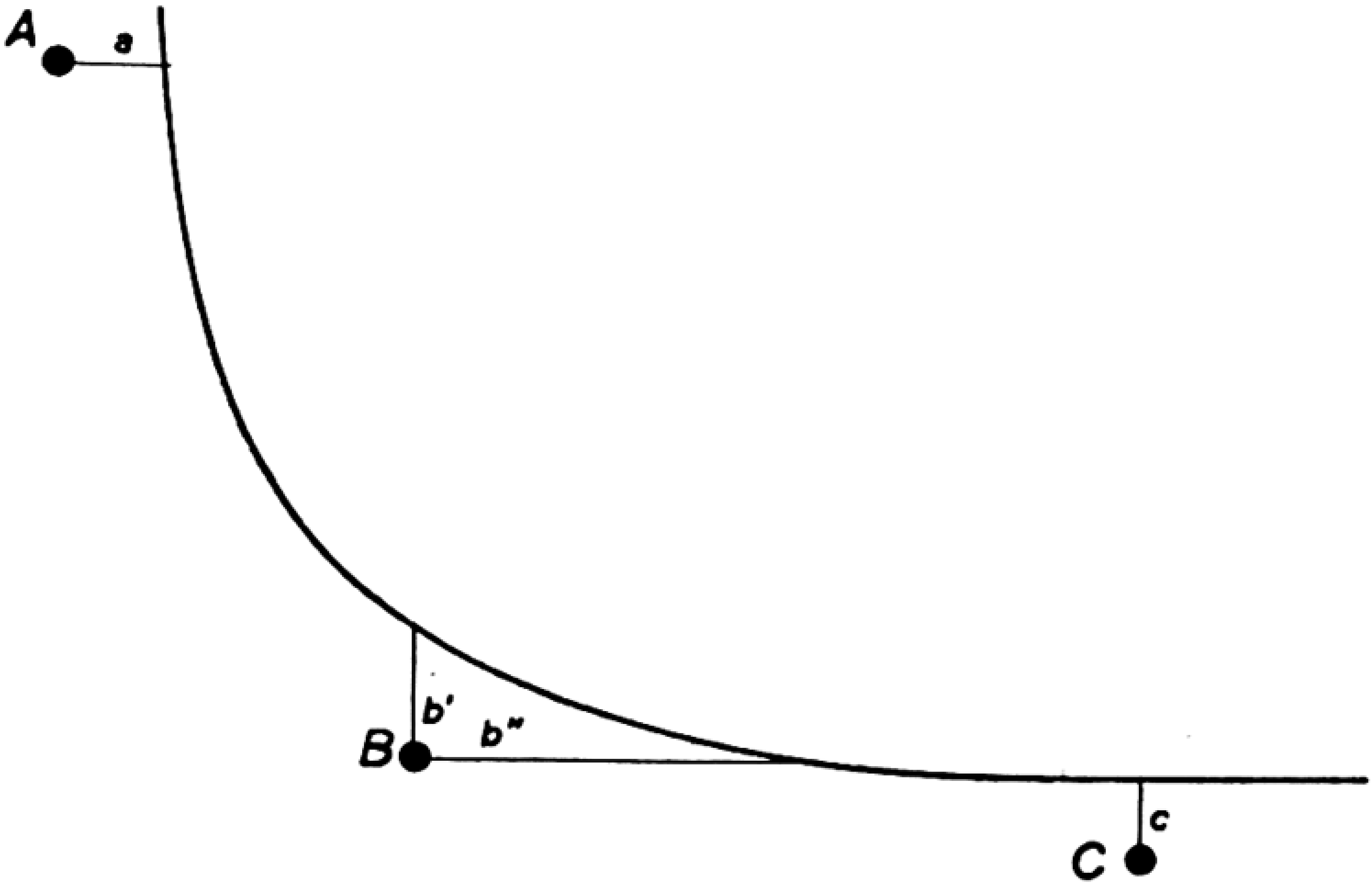

Figure 2. Hypothetical Iversen diagram with three finds of fossil remains. Find *A* indicates a change of winter temperature covering at least the distance *a*. Find *C* similarly indicates a change of summer temperature, *c*. Find *B* indicates either a change of summer temperature, *b'*, or of winter temperature, *b''*, or a combination. The fossil mistletoe in Fig. 1 most probably indicates a change of summer temperature.

The problem now becomes increasingly complicated :

(1) There are limiting values which the regression line approaches asymptotically. These limiting values do, however, make it possible to arrive at absolute values in some cases (see Fig. 2).

(2) The regression is of a much more difficult type than that found by Samuelsson — who only found part of the curve.

(3) Whereas both Samuelsson and Iversen used the summer temperature for one variable, they used different data for the second.

To a certain extent winter temperature and duration of the vegetation period are replaceable, but only within the same climatic district. Thus, the final solution of the problem seems to demand a multiple regression of higher power — needless to say the available data hardly warrant a treatment along strictly mathematical lines*.

This series of examples shows the difficulties inherent in drawing conclusions from palaeoclimatological evidence, even in such apparently simple matters as the meridional displacement of a distribution limit. It should be remembered, too, that this applies not only to biological material, but to abiogenic evidence, *e.g.* snow-lines, as well. Unless the opposite has been proved we must always suspect that there may be — within certain limits — a set of mutually interchangeable factors which can explain our findings. Only in those cases when a phenomenon is observed to have taken place outside the present limiting values, and all alternative explanations can be ruled out, may we safely conclude that the meteorological element in question has changed sufficiently to bring the phenomenon inside the regression line. All this presumes that the ecological demands of the species in question have not changed materially during the span of time under discussion. This is a sore point which is the subject of more firm convictions than can be justified by the number of rational arguments available.

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* I shall not enter upon Enquist's much-debated contribution to the discussion of these problems. It is obvious that his method cannot give more than Iversen's limiting values, and most probably not even those.

Even if a definite statement can be made concerning climatic changes in a certain locality during a certain period, this statement is valid for that locality only. How often do we find a sweeping statement that the temperature of the Ice Age was so and so much lower than at present ? Such a statement may be correct for a particular locality, but the idea of climatic data of universal applicability is meteorologically monstrous. In the above-mentioned paper Buedel has published two schematic diagrams showing the vegetation regions between the north pole and equator, between long. 0° and 15° E. A comparison between them shows beautifully

(1) that the greater extension of glaciers is just one of the phenomena characterising the Ice Age ;

(2) that the corresponding southward displacements of vegetation belts are phenomena of middle latitudes ; and

(3) that in low latitudes the same climatic fluctuation manifests itself, not as a change of temperature, but as a change of humidity.

The Ice Ages of middle latitudes are the Pluvials of the tropics.

Again, this latter concept is far from being generally accepted. What is registered in palaeoclimatology, is not precipitation itself, but precipitation less evaporation, and the latter factor is again a function both of precipitation and temperature. Thus, we may ask : " Are the Pluvials caused by higher precipitation or lower temperature — or by both ? " Provided temperature is higher than a certain threshold value, variations in that element hardly affect tropical vegetation types — and tropical vegetation history can hardly ever be written in more detail than that which uses the vegetation type as a unit. Consequently it is very difficult to obtain any information with regard to temperature changes within the tropical belt. True, the snow-lines of tropical mountains were also depressed during the Pluvials, but again there is the possibility that this was due to increased precipitation more than to lower temperatures. In higher latitudes Ahlmann (1940) has demonstrated that the part played by increased precipitation in glacier advance is insignificant as compared with that played by lower temperature, but it is self-evident that relations for the comparatively dry tropical mountains are quite different from those for the ultra-humid mountain areas around the North Atlantic, *cf.* the part played by evaporation in tropical glaciers.

Glacier measurements (Faegri 1948) in Norway have shown that :

(1) there is no close correspondence between the yearly movements of the individual outlets of the great Jostedalsbre snow-field ;

(2) there is a very close correspondence between the same outlets as regards their reaction to a *c.* 20-year climatic fluctuation, notwithstanding length, exposure, steepness, *etc.* of the glaciers ;

(3) there is the same correspondence with the outlets of the Folgefonnen snow-field, also in South Norway ;

(4) there is no distinct correlation in this respect between the glaciers in South Norway and the outlets from the Svartisen snow-field in North Norway ;
(5) all these glaciers reacted in the same manner to the great fluctuation that culminated with the glacier advance in the middle of the 18th century and the following recession which is still going on ;
(6) this macro-cyclic glacier variation is not in correspondence with similar variations in the Alps ;
(7) both the Alpine and the Norwegian glaciers reacted in the same manner upon that very much longer fluctuation we call the Ice Age.

These findings provide, I suggest, a demonstration of a very important palaeoclimatological principle, *i.e.*: *The shorter the duration of a climatic fluctuation, the smaller is the area similarly affected ; the longer the cycle, the greater the area within which it is felt in the same way.* This should provide a warning not to draw too far-reaching conclusions from local observations. We know very little about the real value of an observation until we have decided whether it represents an accidental maximum of a local cycle, or part of a greater pattern. Unfortunately many observations, especially the earlier ones, are unique, and the conclusions drawn from them are therefore subject to uncertainty. To be able to draw conclusions about a climatic fluctuation we must know both the extent of the geographical area within which it manifested itself, and the length of the cycle. To quote just one example : the present climatic change has so far resulted in lower precipitation in Middle Norway (Trøndelag) and higher in SE. Norway. Obviously the causes of this precipitation minimum in Trøndelag are totally different — at any rate quantitatively — from those of the precipitation minima during the Boreal and Sub-Boreal periods, minima which manifested themselves over great areas. If each of them were known from a single observation only, nobody would be able to judge their relative importance.

A third complicating factor in palaeoclimatology is the slowness of reaction of many indicators. A thermometer reacts upon temperature changes with a time-lag of seconds ; a vegetation type may need centuries to react fully to a change of climate. The time-lag of various indicators varies a great deal, which necessitates a separate treatment of each.

Dendrochronology is based upon the breadth of annual rings of trees. As the name implies, it is primarily an instrument of chronology, but the differences between individual annual rings are obviously climatically conditioned. Dendrochronology was born in Arizona, on the border of the desert, where the dominating factor is precipitation. The influence of temperature is so subordinate that the annual ring curve can be considered a pure precipitation registration. There is practically no time-lag, even the most extreme years hardly influence more than one more year (*e.g.* by conservation of excess water in the soil). Similarly, where temperature is the major factor, one might obtain a pure, synchronous temperature record*.

Glaciers, by their characteristic sediments, are extremely important palaeoclimatological indicators. Their reaction to climate is also comparatively rapid inasmuch as the ablation area will react immediately to the ablation conditions of the summer (which are, unfortunately not synonymous with the temperature of the season). The accumulation area will, however, also react and we know very little about how long time it takes before that reaction manifests itself at the end of the glacier tongue. Consequently there will be a phase difference between the two reactions to the same climatic influence, or rather, the influence of a particular year will be felt at a point in the lower part of the glacier during a succession of years. This will therefore result in a smoothing out of the reaction curve of the glacier. Another point should also be taken into consideration — the position of the end of a glacier tongue is an expression of the equilibrium between accumulation and melting. Even small changes in that equilibrium will most probably result in very great displacements of the end, but the melting-off process is necessarily a slow one. Consequently the changes of size in glaciers are correlated with the climatic changes, but glaciers are hardly ever in equilibrium with the actual climatic conditions. We may, however, presume that climatic fluctuations of a duration of some 15-20 years or more are adequately represented by the size-variation of glaciers, even if we can draw no conclusions as to the amplitude of the climatic fluctuation.

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* Unfortunately, in most regions the tree-ring curve reflects the combined influence of both temperature and humidity, which makes it rather useless for palaeoclimatologic purposes, and at the same time makes it extremely dangerous to use it for chronological purposes. Trees of different habitats (to say nothing of different climatic regions) react differently to the same climatic influence.

Another type of palaeoclimatological evidence is furnished by the *solifluction phenomena* of permanently frozen soil. The fossil solifluction patterns can be recognised and certainly furnish indications of an arctic or sub-arctic climate. Very little is known about the mechanism of, and even less about the condition of formation of such soil patterns. They seem to be in some way related to the number of days with temperature passing 0° (*Crostwechselzahl*, Troll 1943) ; but again we know very little about the time needed for the formation of such patterns under different conditions. It certainly is a relatively slow process, with a time-lag of centuries, and the occurrence of solifluction patterns indicates long-period climatic changes.

Next comes the most important group of palaeoclimatological evidence, *i.e.* that furnished by *organisms* the climatic demands of which are different from those of the present vegetation and fauna of the locality. I have already demonstrated the difficulties and potentialities of such indicators when it comes to deriving absolute data from them. That discussion presumed that the distribution of the organism in question was and is in accordance with its climatic demands. Obviously an unfavourable climate will manifest itself practically without time-lag : demanding species may be exterminated by a single unfavourable season. On the other hand the opposite process is a very much slower one : species near their climatic limits (with consequent reproductive difficulties *etc.*) must colonise ground which is already occupied by other species, the competitive powers of which have not suffered from the amelioration of climate. Pollen analysis has demonstrated the slowness of migration of some species, for instance beech and Norway spruce, which are still very far from their climatic boundaries in the formerly glaciated parts of NW. Europe. Obviously most fatal mistakes can be made by confusing such migration limits with climatic ones. Strictly speaking we are safe only in such cases when the species has withdrawn from extreme limits, corresponding to a more genial climate. The present limit cannot then be very far removed from the climatic one, and the distance between the two distribution limits indicates the minimum value of the climatic change.

In a recent paper Willett (1949) has proposed two palaeoclimatological rules, of which the first, A, deals with " the amplitude of climatic variation relative to length of period. The best evidence, geological, biological and historical, indicates that the amplitude of past fluctuations or oscillations of climate tends to increase with the length of the period of the variation." I am afraid this is a fallacy : what is shown by all evidence is that the longer a period the more complete is the reaction of all indicators. An increase of temperature of 1°C lasting for 1,000 years will produce very marked results, whereas an increase of 10°C lasting for 10 years will pass almost unnoticed. I doubt if any of the Quaternary climatic fluctuations have had a greater amplitude than the difference between individual years. Willett's rule A is in reality an expression of the slowness of reaction of palaeoclimatological indicators, the effect of which is that palaeoclimatological evidence represents an integration of the preceding development for a long period. If that development has for a long time had the same direction, the integrated results will be very distinct, even if the deviation from the " normal " average is rather slight.

It will be understood from the above that I am in complete agreement with Willett's rule B, the principle of the " universal character of all long-period fluctuations of climate." This is the natural consequence of my time area rule.

One rather frequent error results from drawing an unjustifiable conclusion from one biotope to another. The variations of the fauna of the ocean are directly related to variations in the conditions of life in the ocean. We can conclude from the occurrence of *Portlandia arctica* in Great Britain during some part of the Ice Age that the temperature of the ocean bottom was between —2° and +2·5°C (Jensen 1942) but we cannot without reservations assume anything about the temperature of the air from this fact. Owing to the warming up of shallow pools by direct sunshine even in arctic climates the distribution of fresh-water organisms is also a very unreliable indicator of air temperatures.

Palaeoclimatology suffers from the disadvantage that those who can judge the evidence (biologists, geologists, *etc.*) cannot judge the conclusions, and those who can judge the conclusions (meteorologists), cannot judge the evidence. It may be necessary to re-examine the whole problem now and then, even if it results in such a gloomy outlook about the possibilities of translating palaeoclimatological evidence into absolute values as those presented here. Absolute values have long been considered the final object of palaeoclimatology. I think we should to-day try to look further afield : what we need is not a series of individual figures of local significance, but *the circulation patterns of former periods.* To establish them is the primary object of palaeoclimatological research. I feel convinced that once they have been found, many of the problems which baffle us to-day, will find their obvious explanation.

What can paleoclimatology yield to-day ? Fortunately the difficulties are much smaller when it comes to obtaining relative data. Even if we do not know the actual temperature of different periods, we can say — and generally with great confidence — that one period was more or less favourable than another. Even in such cases there are difficulties, which are all too often overlooked by investigators, but they are not insuperable.

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Temperature trends of the past century

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Summary

Temperature records of 50 years or more extent from 129 stations, and shorter records from 54 selected stations, are analysed for the purpose of establishing global trends of temperature during the past century. The trend is found to be significantly upward since about 1885, but by no means uniformly so in all parts of the world. The upward trend is most pronounced in the higher middle and polar latitudes of the northern hemisphere, but apparently negative in the polar latitudes of the southern hemisphere.

The geographical patterns of winter and annual mean trends of temperature by 20-year periods since 1880 are analysed and briefly discussed, together with the trend graphs from a few selected stations. It appears that fluctuations in the intensity and position of the winter polar anticyclones over the North American and Eurasian continents are the most distinctive features of the general circulation pattern indicated by the 20-year temperature changes.

1. Introduction

A number of papers in recent years have dealt with the rising trend of temperature which has been noted during the past 60 years, particularly during the latter half of that period. This rising trend has been most remarkable, and most remarked, at high latitudes in the eastern North Atlantic, but its effects have been noted in many parts of the world. Particularly inclusive of many kinds of observational evidence of this trend is a recent paper by Ahlmann (1948). Notably Brier (1947) and Petterssen (1949) among others have discussed changes of the mean pressure and of the general circulation which have accompanied these temperature changes in certain areas, but to the best of this author's knowledge, there has been made no systematic study of the world distribution of temperature change during the past century.

In previous studies of the general circulation of the northern hemisphere and long-period changes of the world weather patterns, the author noted that the marked positive anomaly of mean virtual temperature which appears in north polar latitudes below the 700-mb level during the 1930-39 decade apparently becomes slightly negative equatorward of 50° N. particularly between 40° N. and 30° N. (Willett 1948). Obviously the world-wide distribution of long-period climatic trends is of paramount significance for any interpretation or physical explanation of the major fluctuations of climate during the historical and geological past. For this reason it was decided to investigate, insofar as readily accessible temperature data may permit, the world-wide temperature trends of the past century. Attention is directed primarily towards the magnitude and periodic character of the temperature trends, to the zonal (latitudinal) distribution of the trend, particularly between the northern and southern hemispheres, and to the cellular pattern of the trend. However, the data are insufficient to extend the study of the southern hemisphere and the cellular pattern of the trend back appreciably beyond the last half century.

2. Source and organization of data

With only two exceptions, the temperature records used for this study are taken from Clayton's *World Weather Records*, thus terminating in 1939. The two exceptions are the two-hundred-year European temperature record carefully prepared by Baur (1948), based on the three stations De Bilt, Berlin and Vienna, and the one-hundred-year record prepared by C. F. Brooks for the Blue Hill Observatory of Harvard University. This latter record, which is extended prior to the establishment of the Observatory in 1885 by adjustment, based on an overlapping period of some years, to the record of a station at the base of the Hill, was selected because of its relative freedom from the influence of any large city.

With the single exception of Blue Hill Observatory and New Haven, Conn., only one record, the best available, is included from each 10° latitude and longitude square. This procedure is followed to avoid overweighting certain areas, particularly in Europe, where the greatest number of extended records of temperature are available. On this basis it is possible to obtain only 129 station records of 50 years or longer. An additional 54 stations with records of between 50 and 20 years were selected to represent 10° coordinate squares from which no longer records are available, for it is desired to study the recent temperature trend on as complete a basis as possible.

The effort was made to select stations with as continuous and homogeneous a record as possible. However, a break in the record or even a change in the location of a station is only a minor obstacle to its use, for the emphasis is placed entirely on the change of temperature from period to period rather than on the departure from normal. A break in the record, or a change in the location of a station necessitates only the omission of one period of change from its record. This procedure obviates the difficulty of combining temperature departures for stations whose normals are computed from different periods or affected by a change of location of the station.

The temperature changes for the year as a whole and for the winter season separately are computed for each station individually by five-year periods, the first and second half of each decade. Five-year changes are represented as occurring between the mid-years of the successive 5-year periods, 1937, 1932 *etc.* For each station individually the temperature trend curve is obtained by plotting the successive 5-year temperature changes from the 1937 pentad backwards. In this manner for each station temperature trend curves are obtained for the annual and for the winter season 5-year changes of mean temperature, referred to the mean of the 1937 pentad as a normal, *i.e.*, as the axis of reference or zero line of the trend curves. Figs. 8, 9 and 10 are examples of such trend curves for individual stations, the full lines representing winter temperature, the broken lines annual temperature. The temperature scale, in °F, is indicated at the top centre of each figure.

To obtain temperature trend curves for the respective 10°-latitude zones, all of the stations in each zone are averaged together by 5-year periods. Since the figures refer to 5-year changes referred to the mean temperature of the 1937 pentad, rather than to departures from normal, the stations can be combined, from one pentad to the next, irrespective of the length of the record or the period of the normal for each station, to obtain temperature trend curves for each latitude zone which are representative of that zone just in proportion to the number of stations (10° squares) which are included. With the exception of a small number of stations in the higher latitudes of each hemisphere, where few stations with long records are available, only stations with records of 50 years or longer are included in the computation of the zonal trend curves.

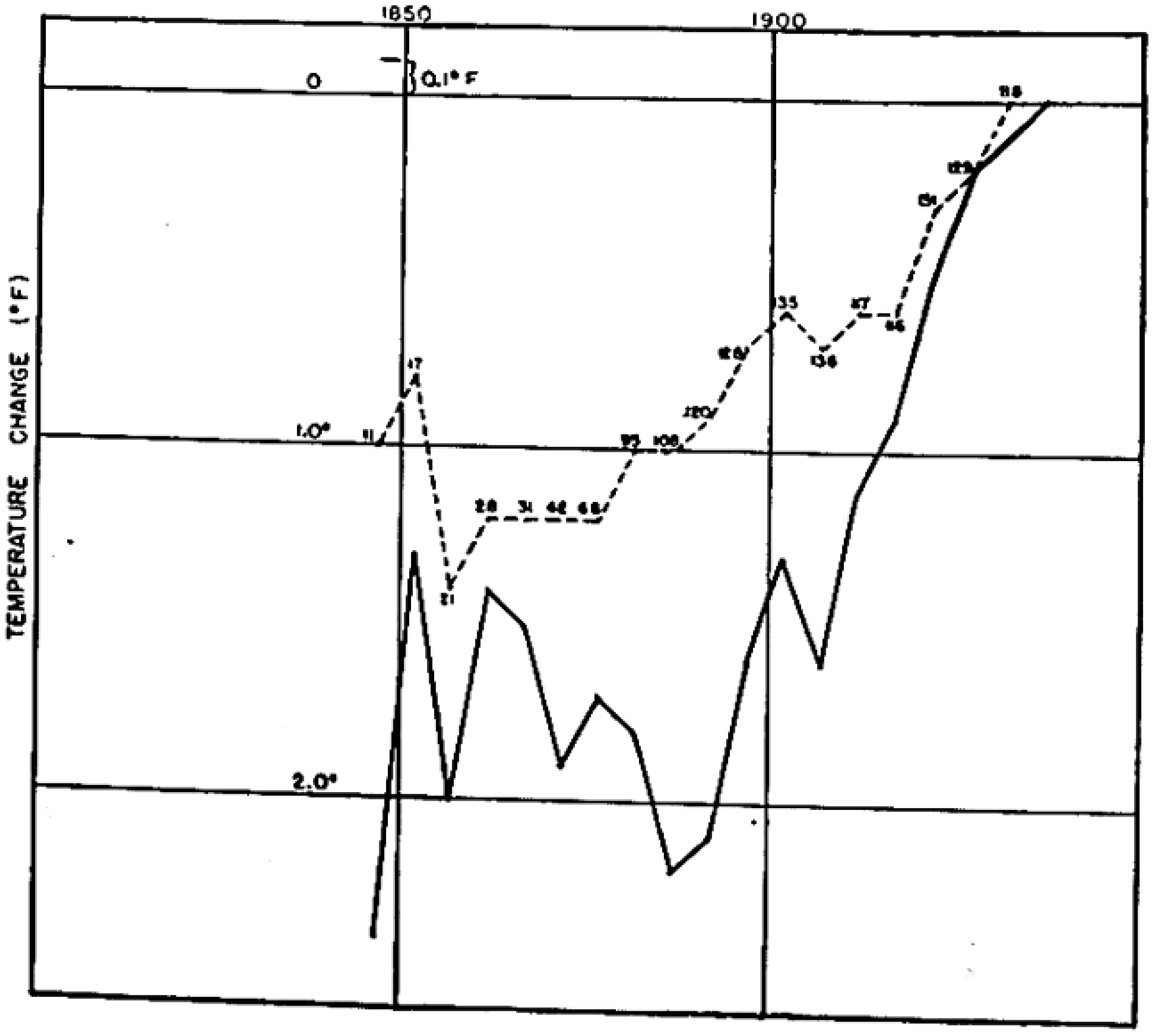


Figure 1. Trend of world mean temperature 1845 — 1940

– – – – Annual temperature ——— Winter temperature

The zonal trend curves for the two hemispheres are contained in Figs. 2 (annual) and 3 (winter season). The latitude designator of each trend curve, which starts at zero for the 1937 pentad, refers to the poleward boundary of the 10° zone represented by each curve. The small figure plotted opposite each pentad point indicates the number of stations which are represented in the 5-year change terminating at that point. The temperature scale in °F again is indicated at the top centre of the figures. All of the stations which are represented in the individual zonal trend curves of Figs. 2 and 3 are combined in exactly the same manner to obtain world trend curves of the mean annual and winter temperature. Fig. 1 contains these two curves, the full line being the winter curve, plotted on a scale ten times larger than that of the zonal curves.

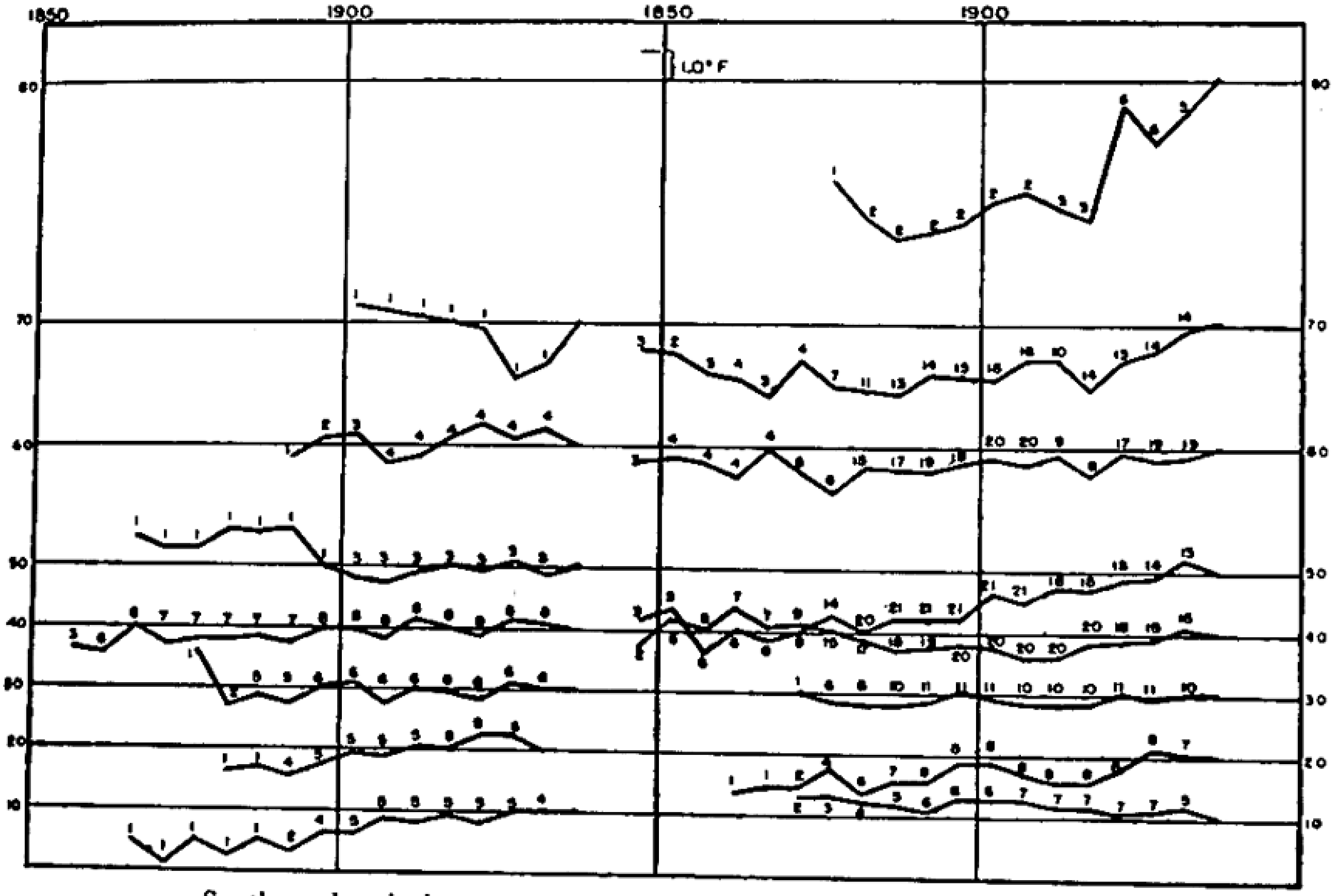
Finally, to obtain a picture of the geographical, or cellular, pattern of the world-wide temperature changes, 20-year changes of temperatures are computed for each station by adding together the four pentad changes centred respectively at 1930, 1910, 1890, *etc.*, and plotting the individual station values for each date on maps. Isopleths of change of temperature are drawn for no change, ± 1°, ± 2°, ± 4° and ± 6°F. The numerical values as plotted are the changes in tenths of a °F for the 20-year period centred at the indicated date. Isopleths are not drawn for any change at a single station which does not fit the general pattern of change, because of the fact that the effect of breaks in the record, or change of station location, cannot be eliminated in computing the long-term change at a single station as it can be in computing successive 5-year mean zonal changes. Thus isolated values appear on these 20-year change maps which obviously are inconsistent with the general pattern of the change.

There are so few stations whose record extends back beyond 1880 that no change patterns are prepared previous to the 1890 period. In general, the change patterns of the winter mean temperature are much more striking than those of the annual mean temperature, as is to be expected. Figs. 4 and 5 contain the changes of the winter mean temperature during the 20-year periods which are centred at 1910 and 1930 respectively, while Fig. 6 contains the change of the annual mean temperature for the latter period.

3. Discussion of results

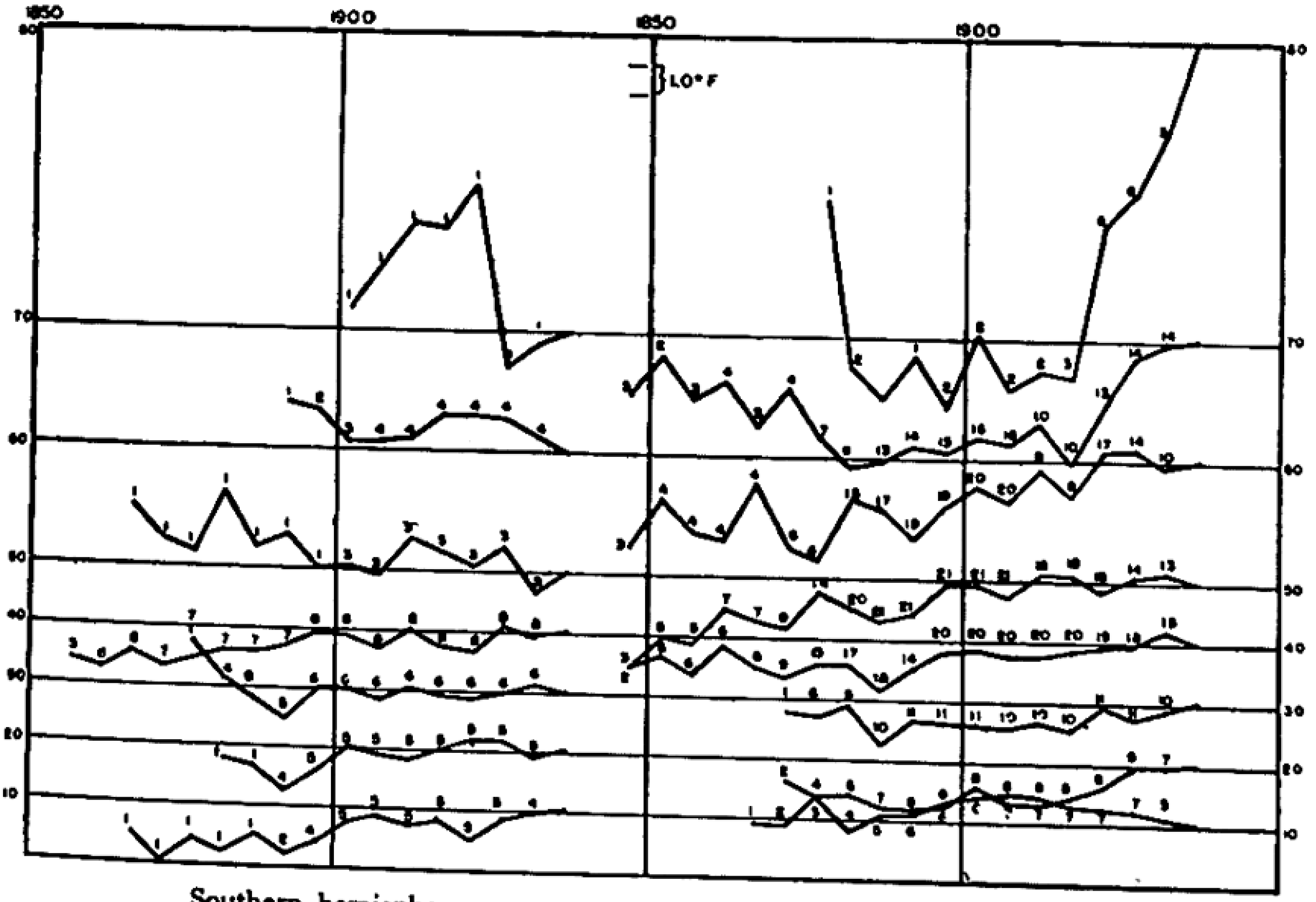
Although most of the figures selected to present the results of this study are self-explanatory, a few remarks concerning them are in order. The outstanding feature of the world trend of mean temperature during the past century (Fig. 1) is the pronounced warming which has occurred since 1885, amounting to 2·2°F in the mean winter temperature and 1·0°F in the annual mean temperature. It is particularly notable that the number of stations remained relatively constant during this period, so that the trend is doubtless real for the area covered by the data. It appears in Figs. 2 and 3, however, that the majority of the stations are from the temperate latitudes of the northern hemisphere. There are only ten stations in the selected list from the southern hemisphere whose records extend back as far as 1885.

From 1850 to 1885 the temperature trend appears to have been downward, particularly for the winter season, but the number of stations included in the earlier



Southern hemisphere Northern hemisphere

Figure 2. Trend of annual mean temperature by latitude zone, 1845 — 1940



Southern hemisphere Northern hemisphere

Figure 3. Trend of winter mean temperature by latitude zone, 1845 — 1940

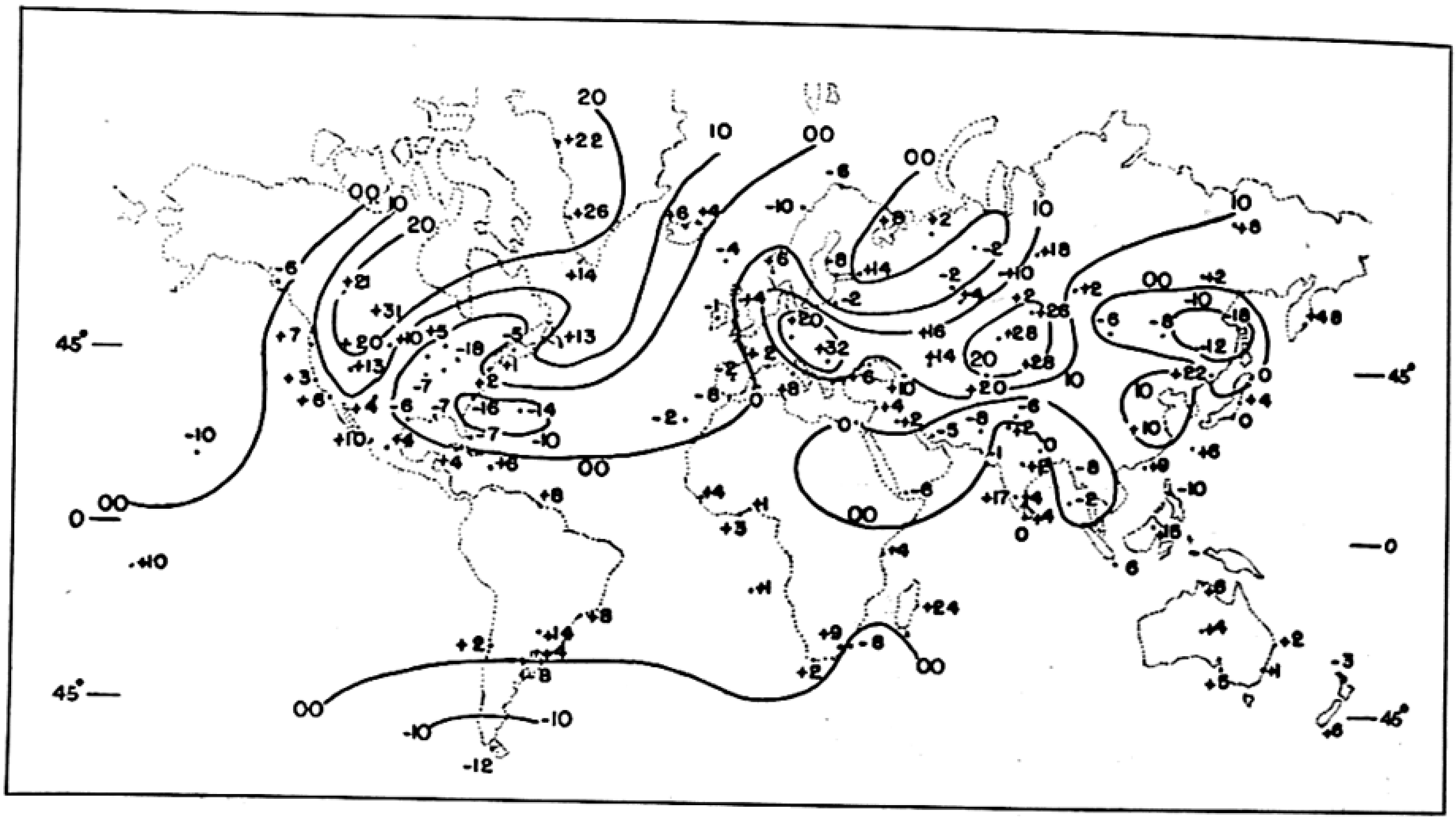


Figure 4. 20-year temperature change, 1910, winter

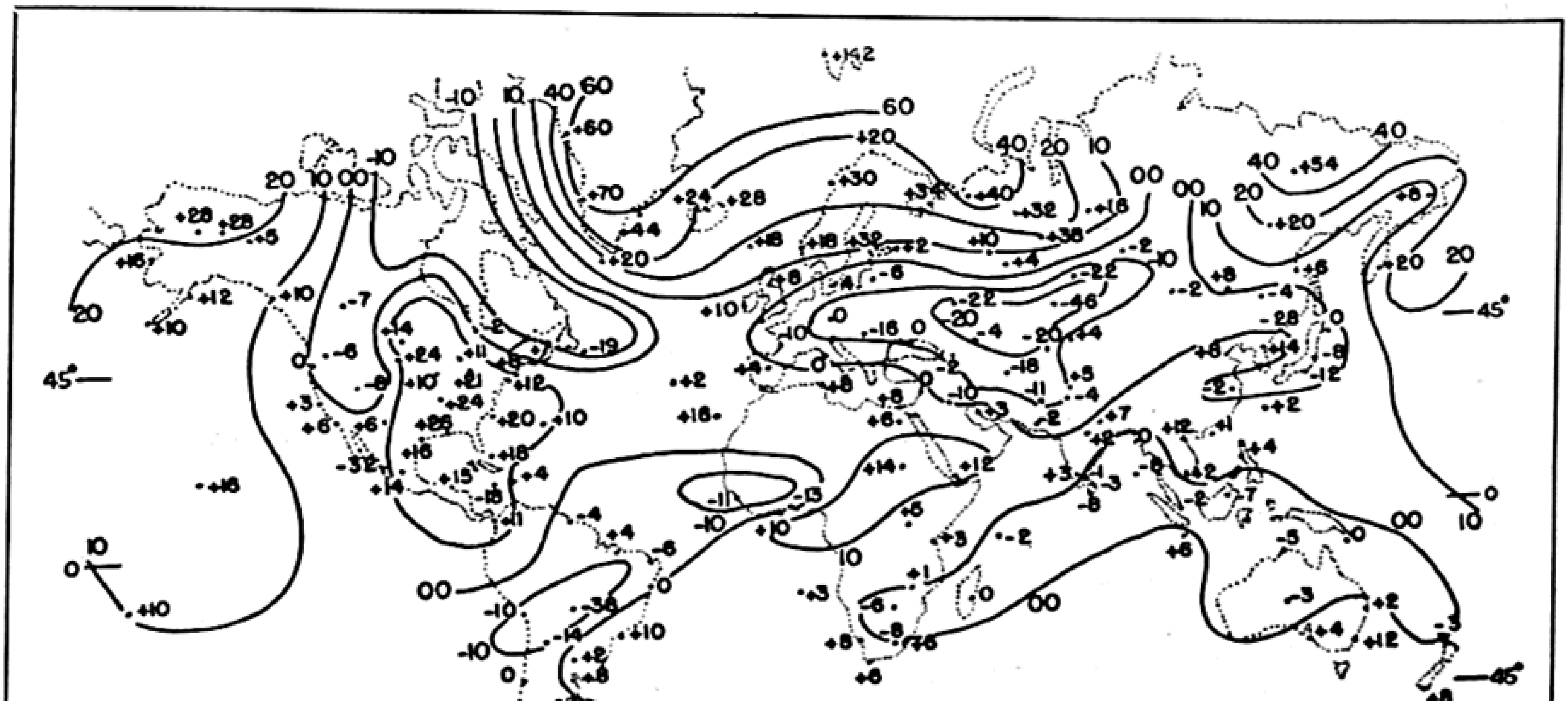


Figure 5. 20-year temperature change, 1930, winter

part of this period decreases rapidly. The sharp increase of winter temperature from the 1847 to the 1852 pentad is based on the record of only eleven stations.

Figs. 2 and 3 indicate the zonal distribution of the upward trend of world temperature since 1885. It is apparent from these zonal trend curves that the warming up has been far from uniform in its zonal distribution, but has been most pronounced poleward of 40° N., and in the first two zones south of the equator (Fig. 2). Particularly noticeable is the fact that the marked rise in temperature in the higher latitudes of the northern hemisphere is not duplicated in the southern hemisphere, but rather, insofar as the data go, the trend is downward poleward of 40° S. This downward trend in the Antarctic agrees with the opinion of explorers of that region that ice conditions have become markedly worse in the Antarctic seas during the past 30 or 40 years. This fact is of significance if it may indicate the possibility of long-term trends on the two hemispheres that are mutually out of phase, a possibility that has important implications for the changes of world climate during historical and geological time, and their possible causes. However, the trend equatorward of 40° S. does tend to parallel that on the northern hemisphere, while the data poleward of this latitude are sadly lacking in number and length of record.

One of the minor inconsistencies particularly of the winter trend curves which is rather noticeable is that of the downward trend of temperature in the zone 0° - 10° N. since 1910 in contrast to the upward trend in the zone on either side, especially in the zone 10° - 20° N. If this difference is real, and there are included enough stations in each zone to indicate its reality, it must be caused by some abnormal position or activity of the intertropical front during the northern hemisphere winter, perhaps a northward displacement, in the more recent years.

Of the three figures which present the geographical patterns of the 20-year changes of mean temperature, Figs. 5 and 6 covering the changes for the period centred at 1930 are of principal interest, by reason both of greater completeness and of numerically greater changes. The most striking feature of both the winter changes and the annual changes is the well-known warming which occurred in north polar latitudes, for which no counterpart is indicated in the southern hemisphere. The winter pattern (Fig. 5) is particularly striking in this respect. The indications are that this rise of temperature in the Arctic is almost circumpolar in extent, but the data, particularly over north-eastern North America, are not sufficient to prove this as a fact. In the absence of data in this region, it was decided to represent in the analysis a break in the circumpolar temperature rise in the Hudson Bay area. The analysis was made in this manner because in other respects this temperature change pattern resembles very closely the temperature anomaly pattern which characterized the past winter, and which is highly typical of a stronger than normal polar anticyclone in western Canada, and a strong development of the subtropical high in the western Atlantic (strong Bermuda high). Under these conditions cold outbreaks occur from north central Canada into the western United States, and south-eastward from Hudson Bay to Labrador, while the eastern United States remains abnormally warm under the influence of the Bermuda high.

It is interesting to note that also in Eurasia the winter pattern of temperature change corresponds to the anomaly pattern of an abnormally strong polar anti-

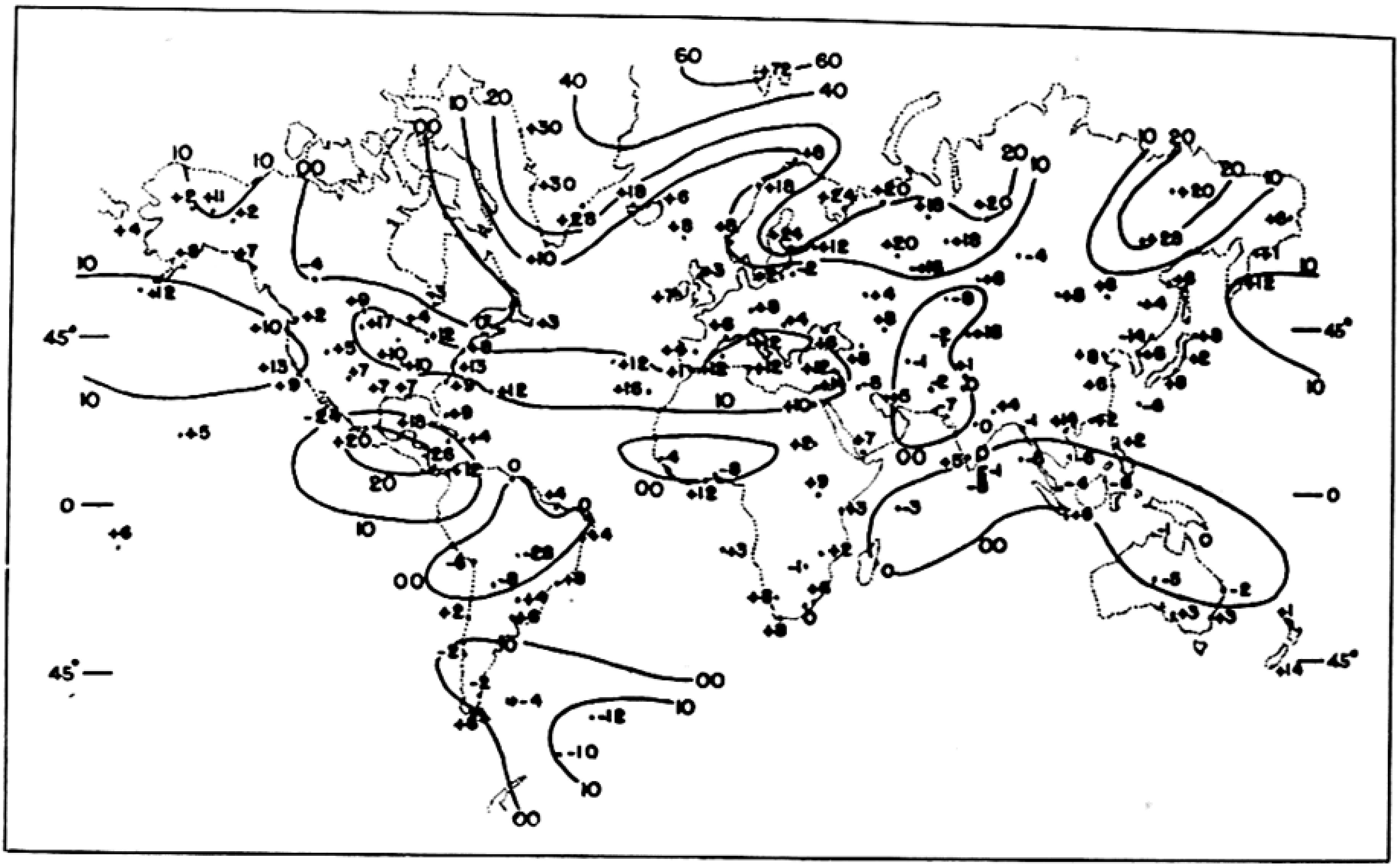


Figure 6. 20-year temperature change, 1930, annual

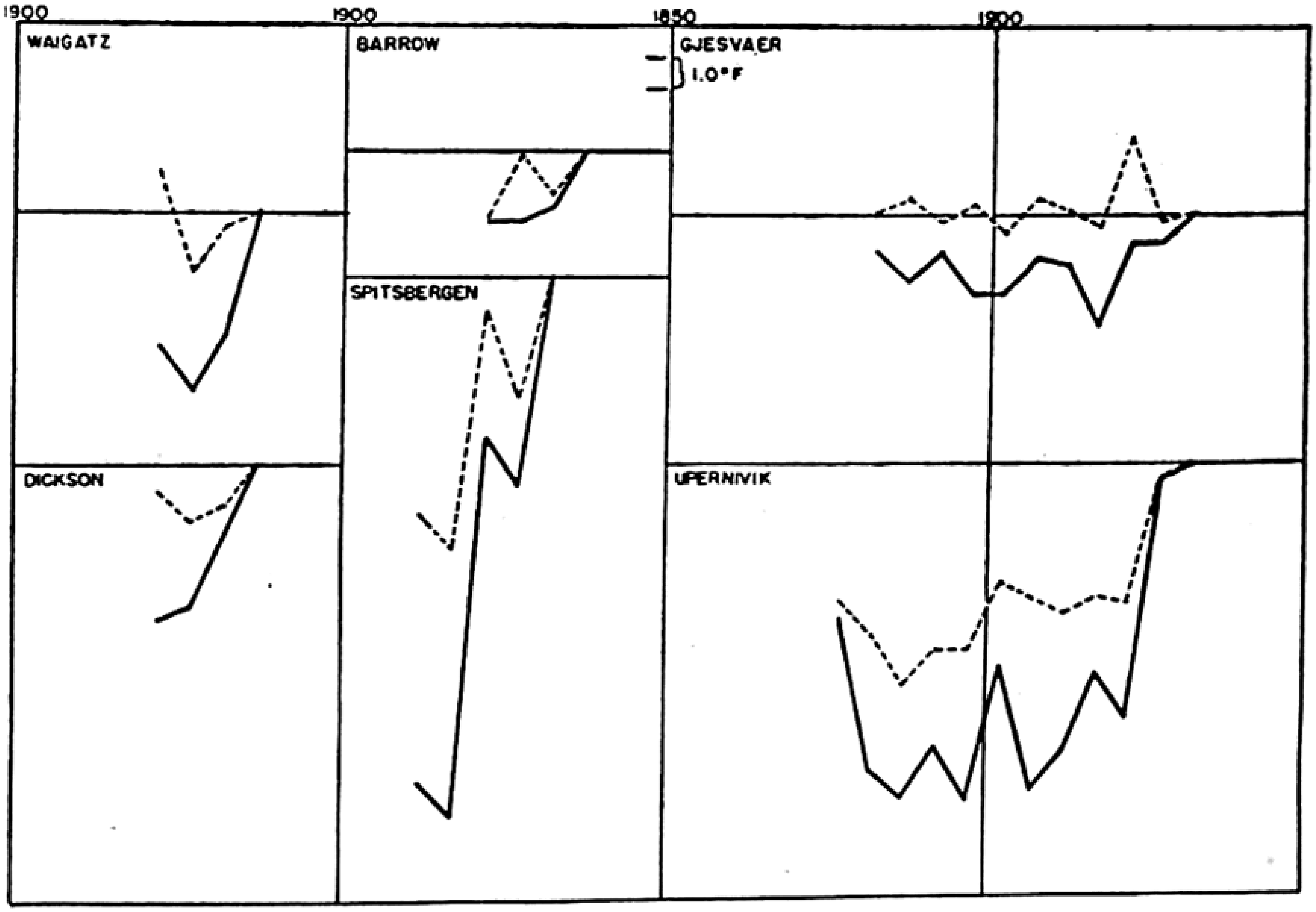


Figure 7. Trend of mean temperature at selected stations

- - - - Annual temperature ——— Winter temperature cyclone probably displaced westward, in central Asia. This feature will account for the abnormal coldness in south-western Asia and eastern Europe, and probably also for the intensified flow of warm air north-eastward in the Atlantic and eastward into northern Siberia. The question legitimately may be raised as to whether the circumpolar belt of marked temperature increase should not, in that case, also be broken in north central Siberia, as in North America, to connect the area of temperature fall in south-western Asia directly with the Arctic in the same manner. Quite possibly this connection should be made, depending upon how far northward the strong Asiatic anticyclone extends, *i.e.*, the extent to which the eastward progress of lows is blocked north of Siberia. Being relatively unfamiliar with the usual circulation anomalies of northern Siberia, the author is hesitant without further evidence to break the circumpolar belt of temperature rise in favour of an arctic outlet in this region, but the analogy with the North American conditions does favour that interpretation of the pattern.

It is interesting also to note that Petterssen's (1949) analysis of the change of mean pressure over Europe and the North Atlantic between the twenty-year period centred on 1910 and that centred on 1930 indicates clearly the westward extension of the Siberian polar anticyclone into north-eastern Europe during this period. Further support of the polar-anticyclone interpretation of the winter pattern of temperature change is furnished by a comparison of Fig. 5 with Fig. 6. It will be noted that the negative winter temperature changes in south-western Asia and northern North America are largely eliminated and partly even reversed in the annual means, a fact which indicates that only the winter season was becoming colder in these areas.

The changes of winter mean temperature from the 1890 to the 1910 period (Fig. 4) are relatively insignificant compared with those of the following period, with few striking features. The data are insufficient to indicate much about the changes in the Polar latitudes on either hemisphere, but it is interesting to note that the pattern of change over North America and central Eurasia is essentially a mild reversal of that of the following period, in particular with regard to the apparent role of continental polar anti-cyclones during the two periods. The corresponding pattern of change of annual mean temperature, for which the figure is not reproduced here, indicates once again, as in the case of the negative temperature changes during the following period in south-western Asia and north-western North America, that the anomaly responsible for these changes is essentially only a winter phenomenon.

The number of stations represented in the pattern of change of winter mean temperature from the 1870 to the 1890 period is insufficient for any real analysis. The only conclusion which can be drawn from the very incomplete pattern is that over Europe and North America it became moderately warmer in the higher middle latitudes, and moderately colder in the lower middle latitudes.

The temperature trend graphs of a few selected stations are presented for brief discussion in Figs. 7-9. Fig. 7 contains the winter and annual trend graphs for all stations with any length of record in the 70°-80° N. zone where the recent extreme rise of temperature has occurred. It will be noted that this rising trend is shown at all stations for the past twenty to thirty years, but that it is primarily a trend of winter temperature, and that it is relatively small and does not appear at all in the annual mean temperature from North Cape eastward. Hence it is not permissible to conclude definitely from only one station in the Antarctic poleward of 60° S. (Laurie Island) that a similar warming up has not occurred there. However, the evidence that this recent upward trend of arctic temperature has been caused primarily by the abnormal development of polar anticyclones in winter over the Eurasian and North American continents makes it seem very doubtful that such a trend has occurred in the Antarctic. An abnormal development of the polar anticyclone over the Antarctic continent probably would not have a warming, but quite possibly a cooling effect on the Antarctic climate, an effect for which there is some evidence in the recent Antarctic ice conditions.

Fig. 8 presents the temperature trend curves for the two stations Blue Hill (near Boston) and New Haven, Conn. Blue Hill is selected as a station which should be particularly free, by reason of its elevation and location, of any local effect of industrial smoke or population growth on temperature. New Haven is the closest industrial town with a long temperature record. Its population has increased eightfold from 1850 to 1940. It will be noted that Blue Hill shows a stronger rising trend of temperature than does New Haven. In general, the largest rising temperature trends have been shown by stations in high latitudes that are far removed from centres of population or industry. Hence there is not the slightest evidence in this study of any significant effect of these two influences on local temperature trends.

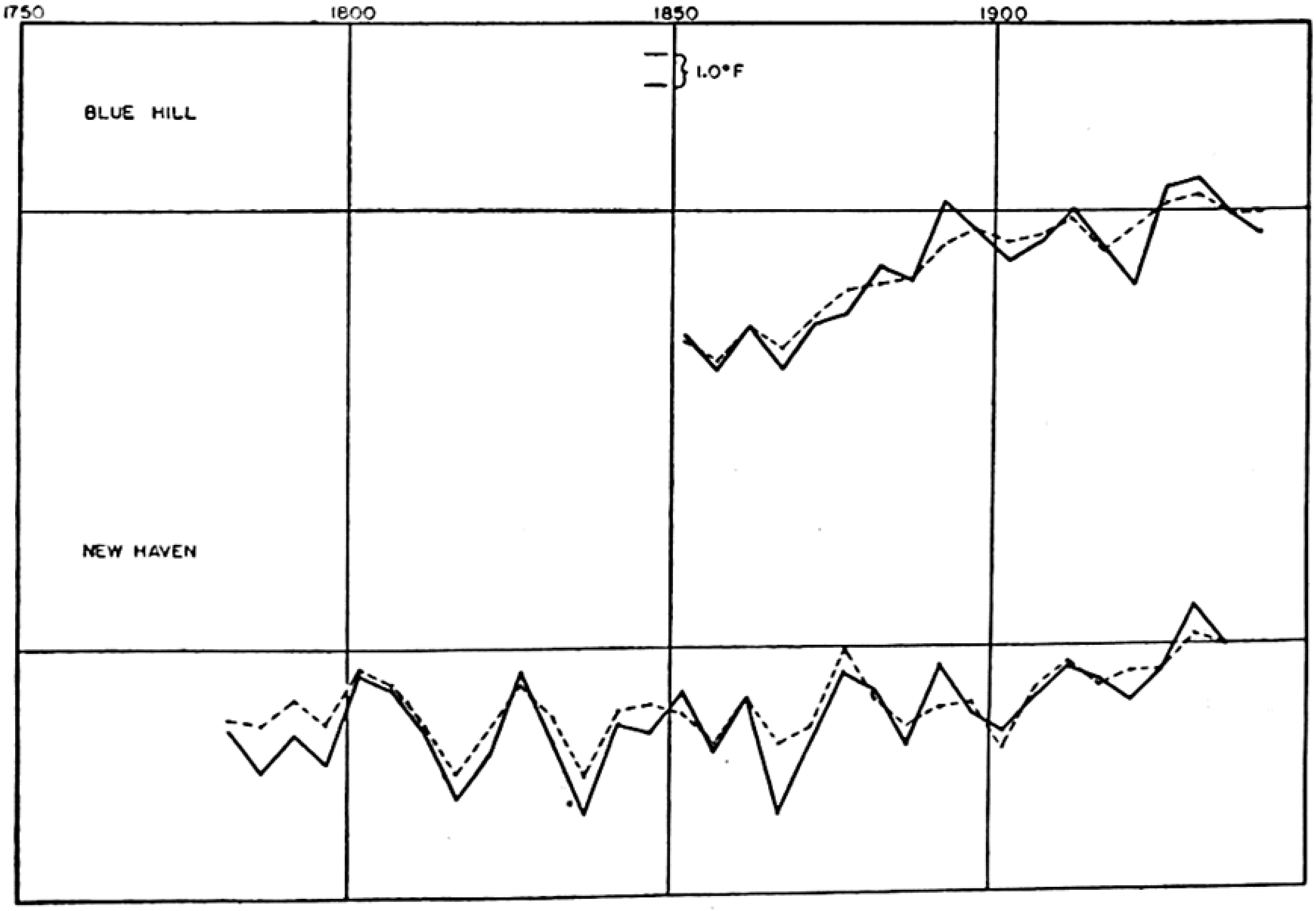


Figure 8. Trend of mean temperature at selected stations

— — — — Annual temperature ——— Winter temperature

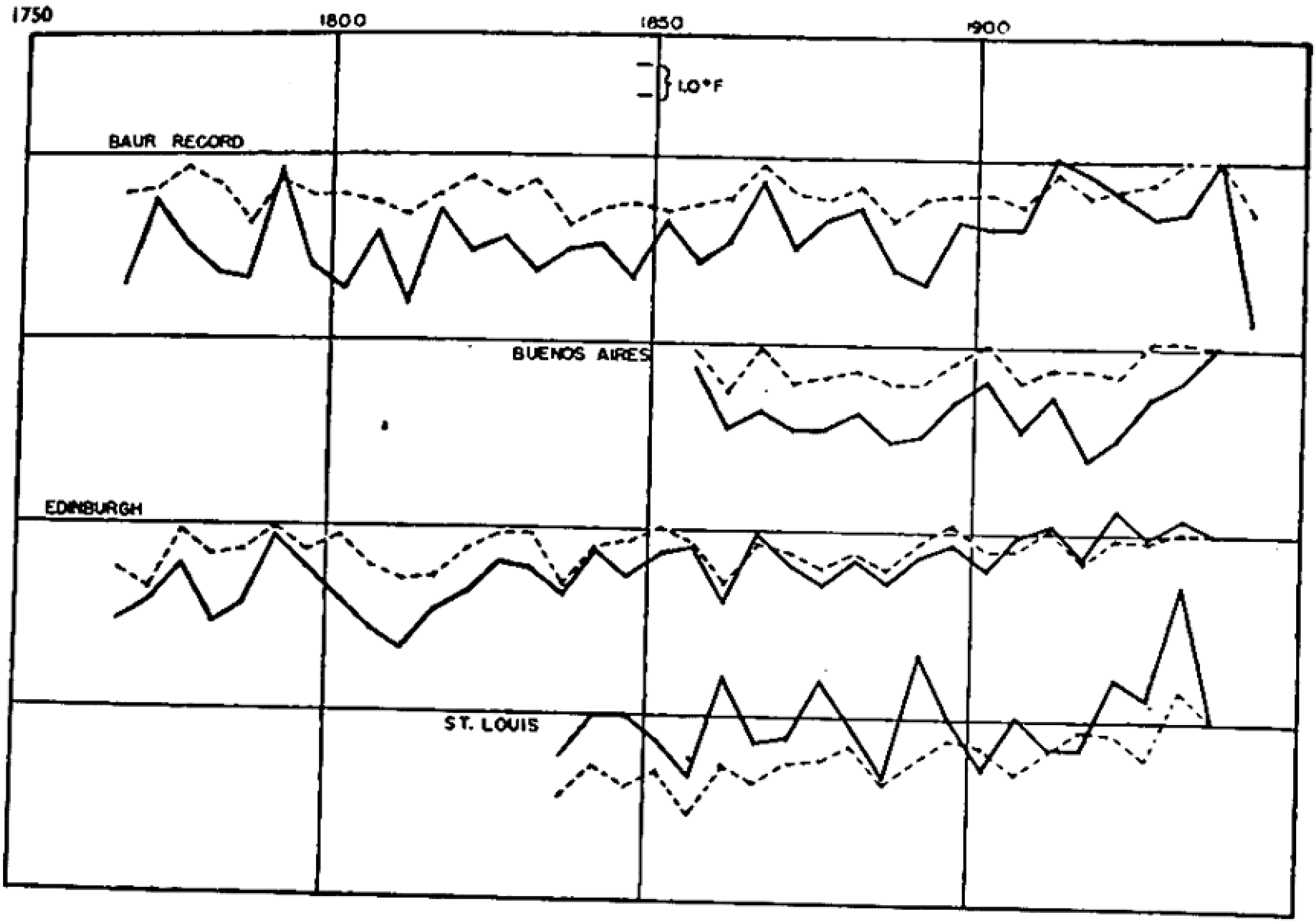


Figure 9. Trend of mean temperature at selected stations
——————— Annual temperature ——————— Winter temperature

The graphs of Fig. 9, together with the New Haven graph from Fig. 8, represent some of the longest homogeneous temperature trend records available. Buenos Aires has the longest record from the southern hemisphere, as has St. Louis from the central or western part of North America. All of these graphs indicate an irregularly rising trend of temperature since the latter part of the nineteenth century, except that Buenos Aires experienced a strong minimum of winter temperature at the 1917 pentad, more or less in phase with the northern hemisphere Arctic stations (Fig. 7). No over-all temperature trend is indicated during the preceding century, although a rather strong minimum during the second decade of the 19th century is shown by the three records which extend back into the eighteenth century. The relative variability of the winter climate under continental compared with maritime conditions is indicated by a comparison of the St. Louis and New Haven winter trend curves with that of Edinburgh. The Baur record, which is the only one to extend through the 1942 pentad, indicates the severity of the three record cold winters which followed 1940.

4. General conclusions

The analysis of the best available world-wide distribution of long records of temperature indicates that a pronounced upward trend has been in progress since about 1885, amounting to 2·2°F in the winter mean temperature, and 1·0°F in the annual mean temperature for all stations analysed. This rising trend has been most pronounced in the higher middle and particularly the polar latitudes of the northern hemisphere, with no counterpart indicated in the higher latitudes of the southern hemisphere. Probably a uniform geographical distribution of station records over the earth's surface would substantially reduce the magnitude of this temperature rise, but not eliminate it. No significant trend of world temperature is indicated during the century previous to 1885.

The geographical patterns of temperature change during the past 60 years indicate quite conclusively that the primary cause of the most significant changes of temperature, in terms of the general circulation pattern, is to be looked for in the intensity and position of the winter polar anticyclones over Eurasia and North America. If this is the most distinctive feature of the changing pattern of the general circulation during recent decades, then it is only natural that no rising trend, parallel to that in the Arctic, but rather a fall of temperature, is indicated in the Antarctic.

There is no evidence that growth of population or of industry is significantly reflected in the records of temperature used in this study.

Acknowledgments

All of the routine work of tabulating, averaging, plotting and graphing of the considerable amount of data which have been used in this study was performed by the personnel of the Weather Bureau, M. I. T. Extended Forecasting Project, as part of the project programme.

Credit for a large part of the work, on which the results discussed in this paper are based, goes to Mr. Murray Mitchell who planned and supervised much of the statistical analysis of the data at the author's suggestion as part of a thesis problem.

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On the present climatic variation

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According to recent investigations (*inter alia* " Recent Climatic Fluctuations," *Folia Geographica Danica*, Tom V, Kobenhavn 1949) the climatic variations which have occurred during the time meteorological observations have been made are so considerable that they cannot be explained as being due to urbanization or erroneous measurements. All elements have been subjected to definite changes or fluctuations.

In Denmark severe winters appear to have become rarer, apart from the last decade, while the mild winters have become more frequent. Cold summers have become rarer and warm ones more frequent during recent years. Apart from the very severe winters, the temperature appears to have risen in the winter, because the number of winter months (January and February) with a negative departure has decreased from 34 in the period 1828-57 to 20 in the period 1918-47, while at the same time the number of months with a positive departure has increased from 24 to 40. Apart also from the very warm summers, the temperature appears to have risen in the summer because the number of summer months (July and August) with a negative departure has decreased from 37 in the period 1828-1857 to 19 in the period 1918-47, at the same time the number of months having a positive departure has increased from 22 to 39. The number of whole years with a negative departure has fallen from 19 to 9 and the number of years with a positive departure has risen from 8 to 21 in the same two periods.

During recent years the very severe winters have become still more severe and the mild winters still milder ; the cold summers have become less cold but the warm ones have not become warmer.

The January-February temperature anomaly chart for 1932 (*op. cit.*, Part III, page 15) shows that the temperature was 4-5°C above the decadal mean in the central part of Siberia and the eastern part of the United States, but 5°C below the mean in Alaska. In the southern hemisphere the summer 1932 was generally warmer than usual.

According to the January-February anomaly chart for 1929 (*op. cit.*) the winter of 1929 was unusually severe and extensive over great parts of the northern hemisphere. In Greenland, North Canada and Alaska, however, the winter was unusually mild, opposite to what was the case in 1932. The summer 1929 was generally warmer than normal in the southern hemisphere. The anomaly chart for January-February 1940 resembles very much that of 1929. The winter cold was unusually severe and extensive in the northern hemisphere. The winter 1940 was especially severe in Scandinavia, Central and East Europe, Siberia and the Central and Eastern States of America, while West Greenland and North Canada had a particularly mild winter. It seems that there exists a connection between certain areas in Europe and North America in regard to climate, and it is probable that this is due to blocking. According to the contours and analysis of the 500 mb pressure level during the mild December 1949 and the cold January 1950 it seems that progressive eastward-moving small and short waves in the upper air are connected with mild winter weather in North-Western Europe, while stationary or retrograde great and long waves with blocking over the British Isles are connected with cold winter weather in the areas around the North Sea and the Baltic.

In the Arctic and the North Temperate Zone the January 30-year normal of temperature has risen during the last century. Many curves resemble each other. In the Arctic regions the rise has been particularly great in recent years (see Fig. 1). For July the rise has generally been smaller and has occurred later.

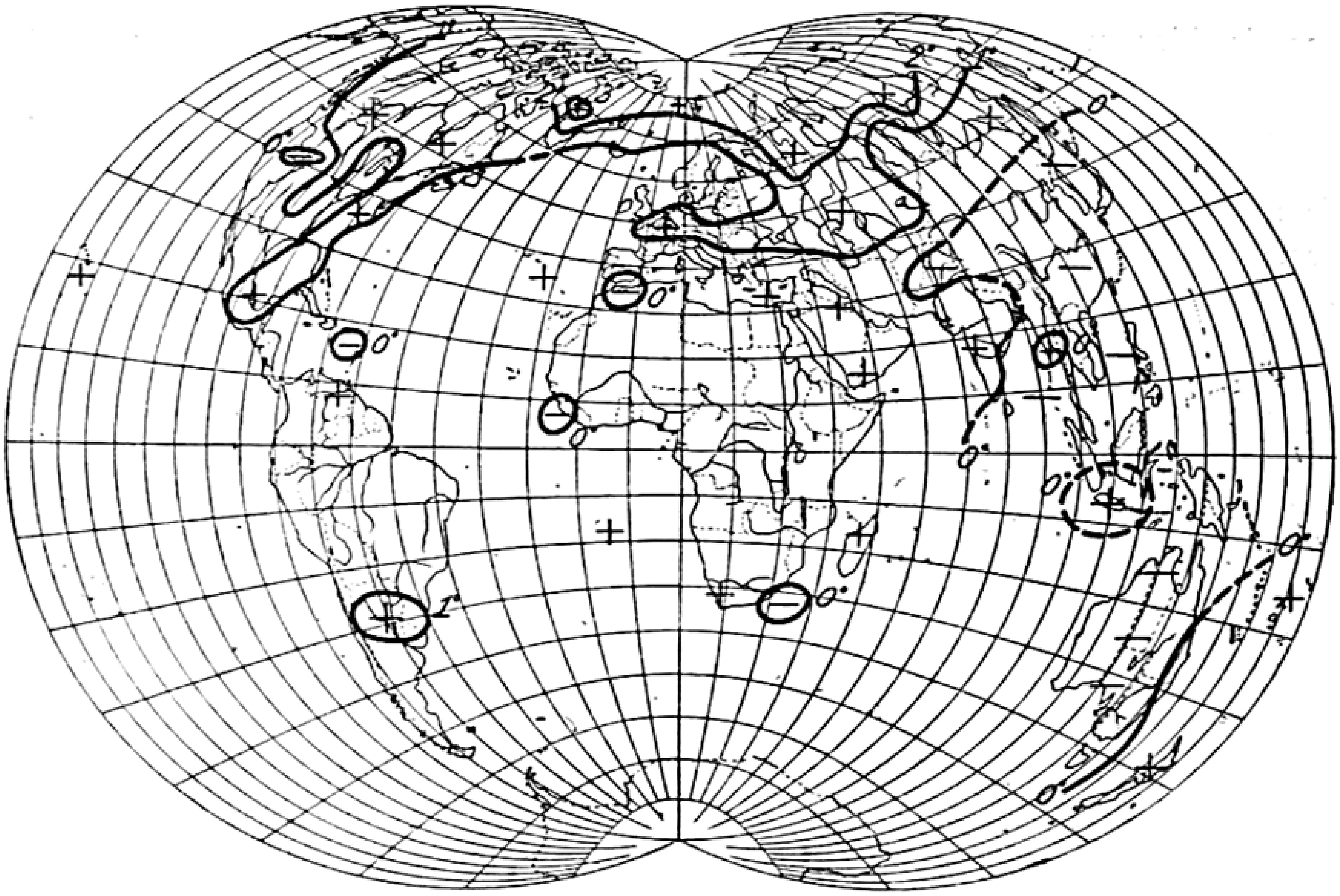

Figure 1. The variation of temperature $T_{1911-40} - T_{1881-1910}$ °C, January.

The temperature variation chart for January shows that the greatest rise of more than 3°C has taken place in Greenland from 1910 to 1940, but Spitsbergen, North Asia and the northern part of North America can also show appreciable rises of more than 2°C. On the whole, the rise in the northern hemisphere appears to become smaller the further south one comes, and even at some places to be succeeded by a temperature fall. The most extensive fall appears to have taken place in East Asia and Australia. In no place, however, does it attain 1°C.

The temperature variation chart for July shows in a similar way that the greatest rise, more than 1°C, has taken place in Finland, North Scandinavia and the central parts of Canada and the United States. It appears that the most extensive fall has occurred in Central Asia and the monsoon districts of South Asia. The north of New Zealand also shows a fall, while Australia and South America have generally had a rise, so that, in other words, the winter in these regions has become milder, as was the case over the most of the northern hemisphere.

The overlapping 30-year normals of precipitation for the year have varied considerably. Only a few curves (*op. cit.*) resemble each other.

The precipitation variation chart for the year shows that the variation from 1910 to 1940 has generally been positive for the Arctic and the North Temperate Zone, in Mexico, at La Plata, in South India and South-East Asia, while it has been negative over the greater part of the United States, the north of South America, Africa, Malaya and Australia. The greatest positive variation, upwards of 400 mm,

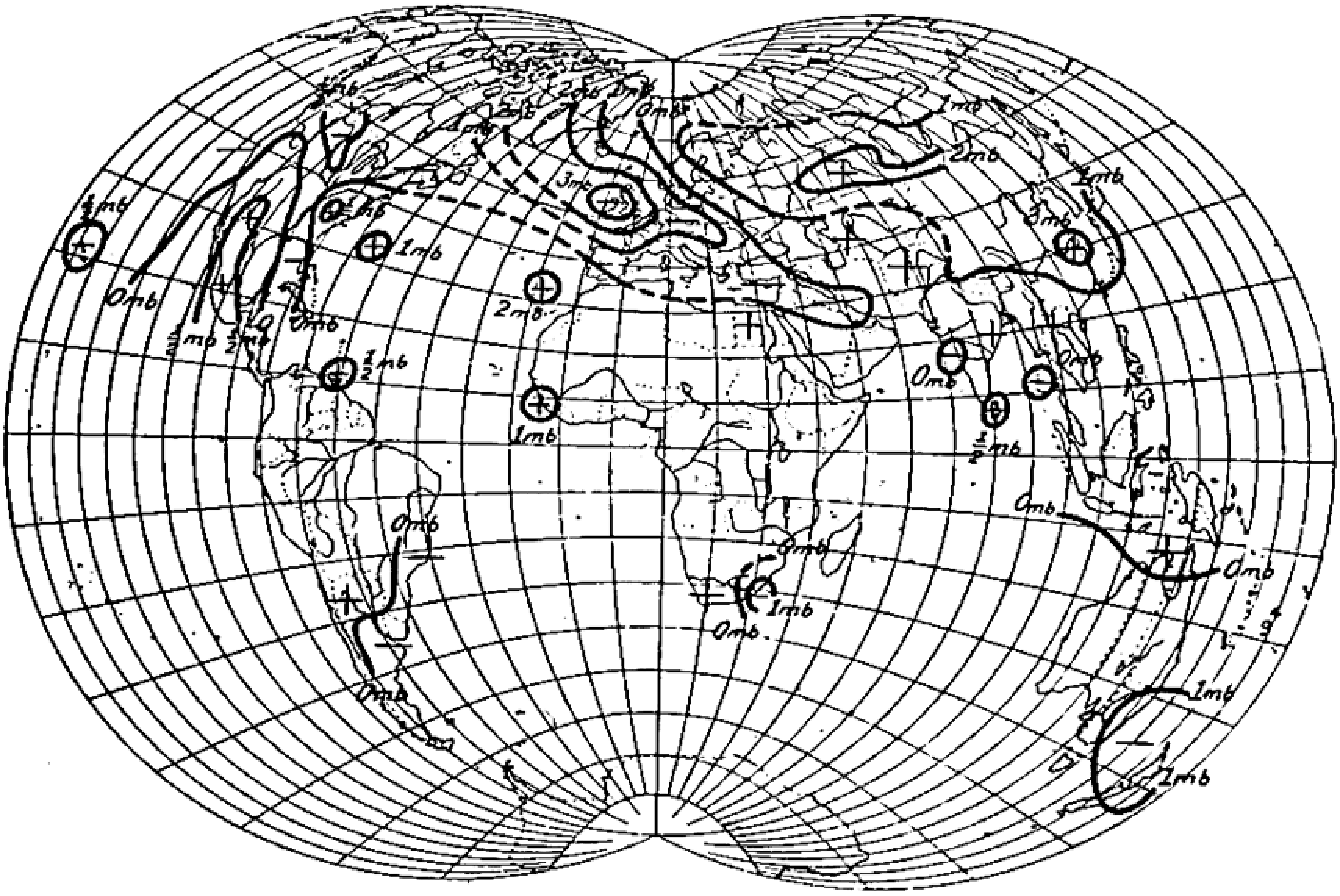

Figure 2. The variation of pressure, $P_{1911-40} - P_{1881-1910}$ mb, January.

is to be found at the Philippines but the greatest negative, upwards of 700 mm, in West Africa.

Also the overlapping 30-year normals of pressure have varied considerably at many stations. Some curves resemble each other.

The pressure variation chart for January (Fig. 2) shows that a significant decrease in pressure has taken place during the 30-years period from 1910 to 1940 over the British Isles, in South Greenland, South-East Africa and South-East Australia, while an appreciable increase has occurred in Siberia and China, and also at Madeira and Bermuda. In short, the pressure has become lower over the northern part of the North Atlantic but higher over the southern part, where the Azores high pressure appears to have become still higher and more extended. On the other hand the winter high pressure over Siberia appears to have become still higher also. On the whole the pressure in the north temperate zone appears to have become higher over land and lower over water in winter, just as it appears to have become lower over land in summer in the south subtropical and temperate zones. It is noteworthy that there is no great fall in the pressure on the west coast of Canada, corresponding to the great fall over the British Isles. Perhaps the fall may be found over the eastern part of the North Pacific.

According to the July variation chart a considerable decrease of pressure has occurred in West Europe, Central Asia and South Australia but an increase over the Atlantic Ocean between West Africa and North America, over the Rocky Mountains, La Plata, New Zealand and Japan. In the northern hemisphere the pressure in the temperate zone, on the whole, seems to have decreased over land but increased over water in summer, and the pressure in the temperate zone in the southern hemisphere seems to have decreased over land in winter.

The variation charts indicate an increase of the South-East wind over the area around the North Sea and the Baltic.

The overlapping 30-year normals of pressure differences for pairs of stations show that the geostrophic wind may have varied considerably over great parts of the earth. For January the west wind at the turn of the century became somewhat more dominating than previously over Western Europe and South Scandinavia. In about 1910 and in the twenties the west wind was particularly strong, but in 1935, five years prior to the three severe winters of 1940, 1941 and 1942, a sudden decrease occurred in the velocity of the west wind, a condition which was so evident that it was reflected in the daily weather charts in the following years. Over South Scandinavia the south wind decreased until shortly after the turn of the century, after which it remained light to about 1920, when it began to increase, to reach by 1940 approximately the velocity it had before. For Western Europe the west wind has increased considerably in velocity, especially after 1915, but, just as was the case for South Scandinavia, the west wind also appears to have culminated in Western Europe shortly after 1935. The pressure differences between Funchal and the equator, represented by Batavia, have generally been increasing, particularly since the twenties, which means that the north-east trade-wind has been stronger since that time. It decreased a little in velocity, however, in 1939.

For January the pressure differences indicate a significant increase of the transport of Atlantic air masses to Europe in winter. The variation of pressure in the Iceland- Greenland region is very important, not only for these two countries but also for the North Atlantic and Europe. It appears that the variation has been of such a character since about 1917 that the pressure has been higher in South-West Iceland than in South-West Greenland. But as soon as the mean pressure becomes higher in South-West Iceland than in South-West Greenland, the south-east wind will become the prevailing wind between Iceland and Greenland. The importance that a change of wind from north to south can have for the climate, the ocean currents and the fauna and flora of these areas is very great.

Overlapping 30-year normals of frequency of wind directions show that the frequency of the south-east wind has increased both for January and July over Western Denmark, and nearly the same has been the case for De Bilt as regards the winter and summer.

The curve for overlapping 30-year normals of sunspot numbers shows an eleven-year period and probably also a period with a wavelength about 80 years. According to the temperature anomaly charts it is evident that the correlation coefficients for the temperature and sunspot-numbers may vary from region to region, if there exists any connection between air temperature and sunspots at all. For the smoothed values the overlapping 30-year normals, the following coefficients C show the connection between the variation of sunspot-numbers and the variation of temperature, precipitation and pressure at Copenhagen, Edinburgh and Stykkisholm.

Correlations (c) of the variation of sunspot-numbers with the variation of temperature, precipitation and pressure

Sunspot-numbers	Station	January c	July c	Year c	No. of years
Temperature	Copenhagen	—0·32	—0·39	—0·63	120
Precipitation	Copenhagen			—0·45	91
Pressure	Copenhagen	—0·34	0·52	0·30	70
Pressure	Edinburgh	—0·41	—0·45	—0·14	140
Pressure	Stykkisholm	—0·37	—0·64	0·46	61

551.583.3 : 551.35

On the interpretation of climatic variations as revealed by a study of samples from an equatorial Atlantic deep-sea core

By C. D. OVEY

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Summary

Preliminary results of a study of the variation in sea-surface temperature from counts of planktonic foraminifera in an equatorial Atlantic deep-sea core show that climatic changes can be detected from the topmost 55 cm of core. A determination of the amount of error in counting was made and was found to be small. Temperature changes, as indicated by temperate and warm-water foraminifera, appear to bear a direct relationship to the CO_2 content of the samples.

Introduction

There is no doubt that cores obtained from the floor of the deep-sea, where sedimentation has probably been continuous and uninterrupted through many geological periods, will afford opportunities to study the most complete chronological record of the geological and climatological history of the oceans. In respect of the latter it has been clearly demonstrated in a recent paper (Wiseman and Ovey 1950, p. 58) that a study of bottom samples containing the accumulated calcitic ($CaCO_3$) tests of certain species of free-swimming foraminifera, which lived in the surface layers of the ocean, can give a reflection of sea-surface temperatures. These in their turn are a function of air temperature. Since most of the species have a geological range well back into the Tertiary (Wiseman and Ovey 1950, p. 59, Table I) it might reasonably be assumed that the fossil contents of ocean cores will, in the same way, reflect temperature changes of the past. The possibility of a species changing its tolerance to temperature conditions is unlikely. Any such change, however, would soon be recognised by checking the occurrence of the species in question against the remaining species used in each temperature group, for it is improbable that a large number would alter their ecological characteristics simultaneously.

Swedish deep-sea Expedition 1947-48, *core* 241

The species used in the study of the core in question are listed and grouped into three categories (cold, temperate and warm) in accordance with the recent work cited above (p. 60, Table II), and the method of counting approximately 1,000 specimens per sample is the same as that used in the validity test. The core was taken with the Kullenberg piston core sampler (Kullenberg 1947) at 01°10′N., 19°50′W. under 4,350 m (2,379 fm) of water in an area of the sea-floor which is relatively flat, thus avoiding the possibility of gravitational sliding which would have disturbed the steady accumulation of sediment. The total length of the core is 14·402 m of which the top 55 cm (12 samples) only have so far been studied.

In order to determine the sampling and personal error involved in the examination of the samples two were recounted on a new spread of material. The results of these tests are given in Table I, and it will be observed that the variation is small.

TABLE I

Sample No.	Depth from top of core	Count	Percentage occurrence of species			Percentage deviation in warm species
			cold	temperate	warm	
3	9·5 — 10 cm	1	—	2·9	97·1	0·9
		2	—	3·8	96·2	
9	39·5 — 40 cm	1	0·3	19·4	80·3	0·8
		2	0·4	20·1	79·5	

Before interpreting results it is necessary to have some idea of the number of specimens to be counted in order to obtain a representative sample. In Fig. 1 the percentage of warm specimens as the count proceeds is plotted against the total number in sample No. 9 for the two counts. It will be seen that the two curves converge. At about 315 specimens the deviation between the two counts is 2 per cent. After about 400 specimens the difference is never more than 1 per cent. It is reasonable therefore to assume that the allowance of 1·5 per cent deviation is sufficient and that any changes in the fauna of two successive core samples showing a difference greater than 1·5 per cent can be regarded as authentic. Conclusions based on small counts (say smaller than 300) are likely to give erroneous results. In this connection it should be mentioned that F. B. Phleger (1948) has recorded eleven samples with less than 300 specimens in his analysis of core 34 in the Caribbean Sea (14°25′N., 65°55′W.), the first of the Expedition's cores to be examined.

The topmost 55 cm of core 241 indicate an extreme of 18·7 per cent variation between the warmest and coolest samples in the case of the warm water indicators. The percentage of cold indicators was always less than 1 per cent (see Table II), so that the temperate curve is nearly an exact reciprocal of the warm. The curves are plotted in Fig. 2, and the sample at 0°1′S., 15°16′W., analysed in Wiseman and Ovey (1950, sample No. 6), is taken as representative of present-day conditions

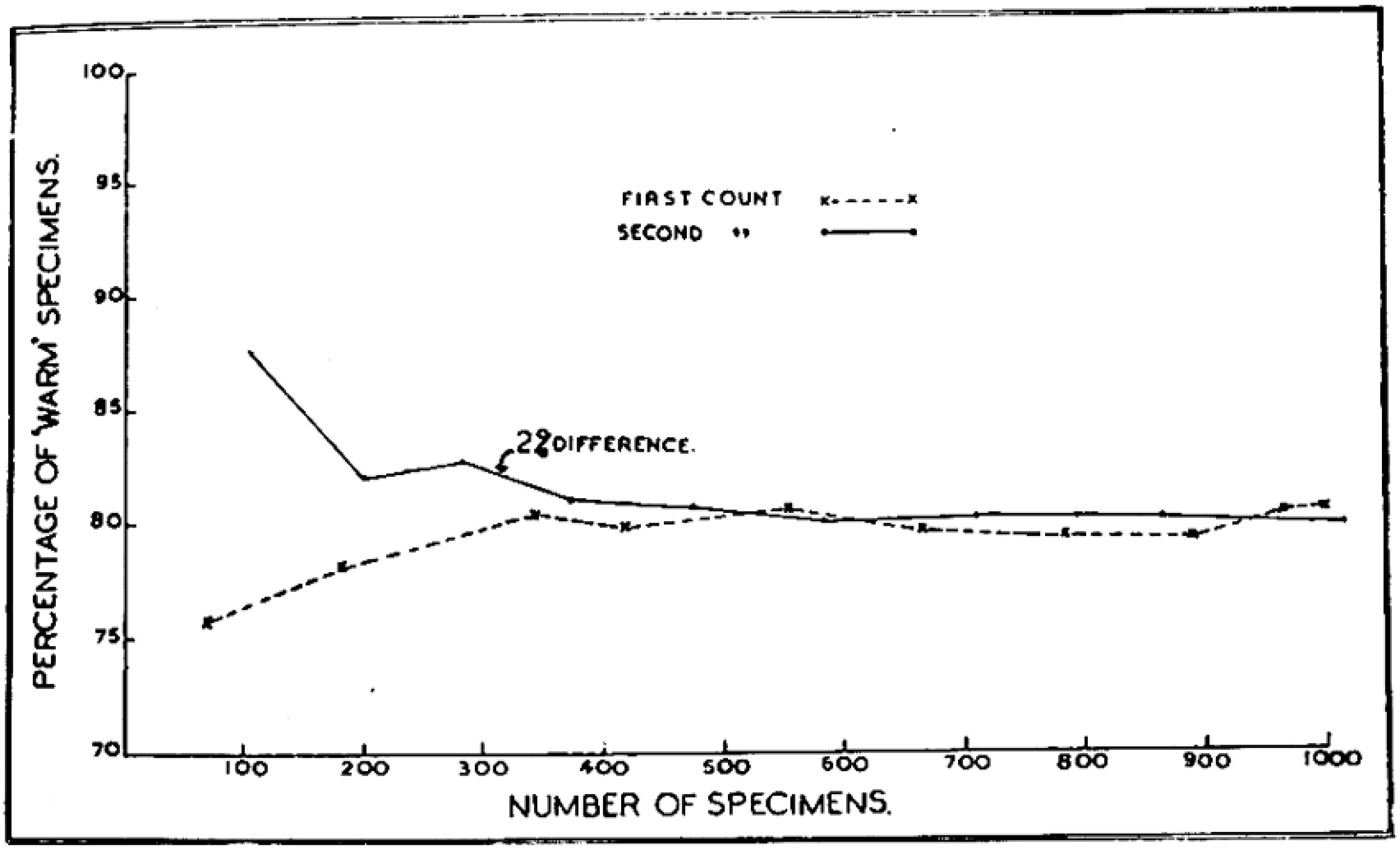

Figure 1. Test for error in the foraminiferal analysis of Swedish deep-sea Expedition 1947-48, core 241, sample 9. Two counts are plotted, each representing a separate examination of different spreads of material from the same sample. A count of less than 300 specimens is not regarded as reliable.

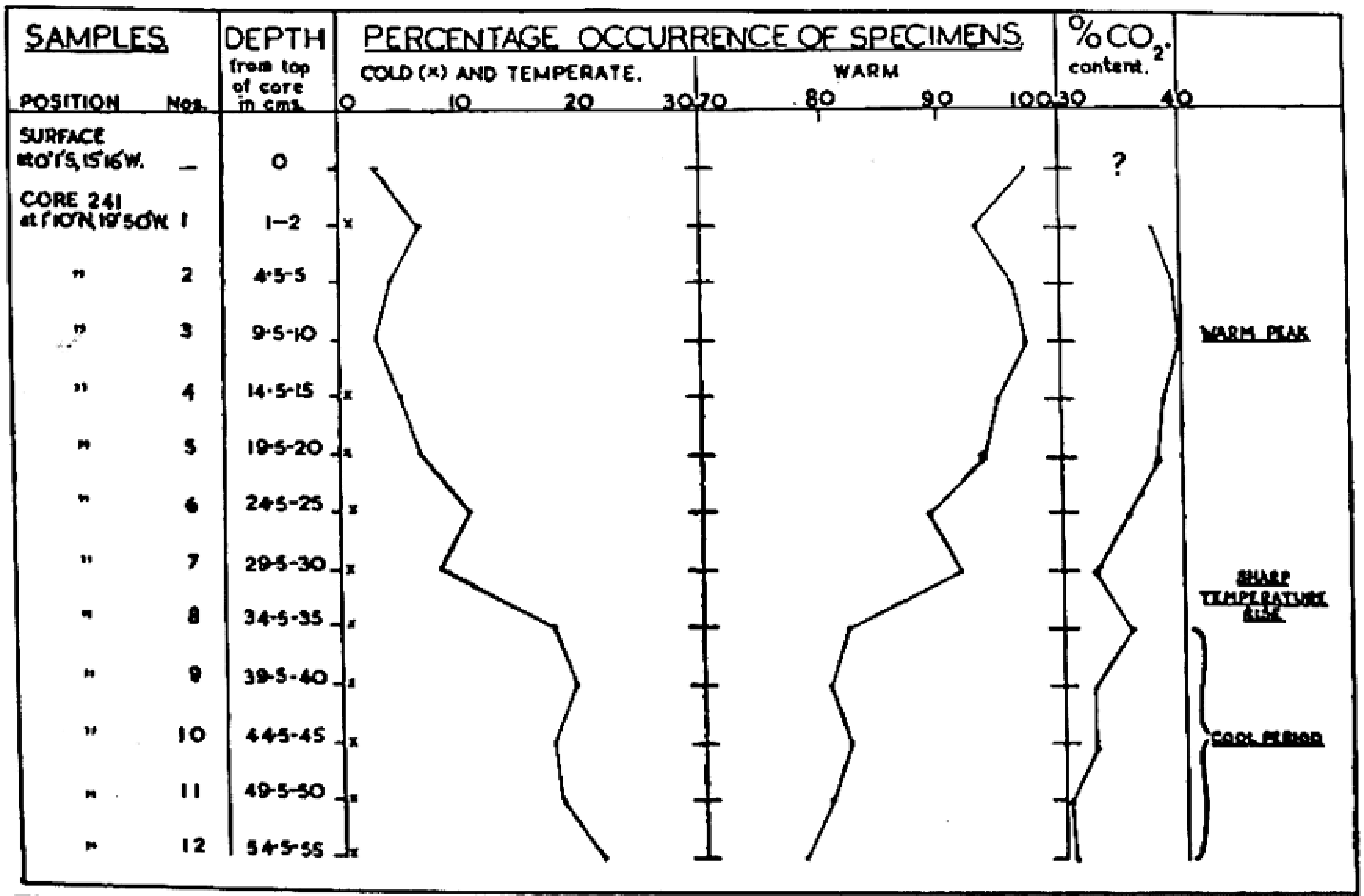

Figure 2. Foraminiferal analysis of the first 12 samples of Swedish deep-sea Expedition 1947-48, core 241 in the equatorial Atlantic. The percentage of carbon dioxide by weight, plotted on the right, shows close agreement with the warm-water foraminifera curve. The cold forms were never more than 0·7 per cent of the total counts of samples.

as revealed by the surface of the floor of the ocean in that region. It is not known how much gap there is between sample No. 1 at 1-2 cm and the actual level of the deep-sea floor because some material is always lost in the extraction of the core owing to its very fluid condition at the top. It is hoped to be able to determine this by correlating a short pilot core, not yet examined, made alongside the parent core at the same time.

It is interesting that the analysis of the CO_2 content now being carried out under the supervision of the writer's colleague, Dr. J. D. H. Wiseman, so far shows a striking agreement with the foraminiferal counts (see Table II and Fig. 2). It will be noted that, as a rule, the samples indicating higher surface-water temperatures have a higher percentage of CO_2 than those that suggest colder surface waters. Other analyses will be applied to the sample which may be a further check of temperature changes. The radioactive age-determinations (Piggott and Urry 1942) are being undertaken by Professor Hans Pettersson, the leader of the expedition, who kindly sent the core in question to the British Museum (Natural History) for examination.

It is impossible to suggest the range in variation of temperature of the sea surface implied by the work at this stage but it seems unlikely, judging from the work already completed (Wiseman and Ovey 1950, p. 58 *et seq.*) that it will be very great. It is however tempting to speculate that the bottom five samples (Fig. 2) belong to the last phases of the Wisconsin (Würm) glaciation. It may be, too, that sample 3 comes from sediments laid down in the Climatic Optimum. One thing is certain, however, that there are oscillations in the equatorial Atlantic. Similar oscillations occur in shorter cores taken by the *Meteor* Expedition 1925-27 (Schott 1935). Finally it must be emphasised that short-term fluctuations of temperature are unlikely to be traceable in deep-sea cores because sedimentation is slow and there is also the probability that the lag between temperature and faunal change is considerable.

TABLE II. PRELIMINARY RESULTS OF TOPMOST 55 CM OF SWEDISH DEEP-SEA CORE 241

Sample No.	Depth from surface	Percentage occurence of foraminifera			Percentage CO_2 content (by weight)
		cold	temperate	warm	
—	* Surface	—	2·9	97·1	—
1	2 — 3 cm	0·6	6·5	93·0	37·4
2	4·5 — 5 cm	—	4·0	96·0	39·2
3	9·5 — 10 cm	—	2·9	97·1	39·9
4	14·5 — 15 cm	0·4	4·9	94·7	38·4
5	19·5 — 20 cm	0·1	6·4	93·5	38·0
6	24·5 — 25 cm	0·7	10·4	88·9	35·4
7	29·5 — 30 cm	0·4	8·3	91·3	32·8
8	34·5 — 35 cm	0·7	17·3	82·0	35·6
9	39·5 — 40 cm	0·3	19·4	80·3	32·5
10	44·5 — 45 cm	0·4	17·5	82·1	32·6
11	49·5 — 50 cm	0·3	19·4	80·3	30·3
12	54·5 — 55 cm	0·1	21·5	78·4	30·9

* Sample taken from surface of sea floor at 0°1′S., 15°16′W. (see Wiseman and Ovey 1950).

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Discussion

MISS N. CARRUTHERS (*Meteorological Office*) : The ideas that I put forward here are due to Dr. C. E. P. Brooks.

It is generally accepted that the most recent glacial epoch, the Quaternary, was the result of mountain building in the late Tertiary. There is however, a lag of about a million years between the peak of mountain building in the late Pliocene (last phase of the Tertiary Period) and the commencement of glaciation. To account for this lag, the theory has been put forward that mountain building merely set the stage for glaciation and that some other cause was responsible for actually starting it off. As the most likely external influence is solar radiation, it is concluded that there must have been a change in solar radiation at the time when ice sheets first started to develop. This somewhat unscientific line of argument has not, as far as I know, been substantiated. Major upheavals of the earth's crust are believed to have occurred at fairly regular intervals during the past 1,000,000,000 years as indicated in Fig. 1 based on geological evidence. All these epochs of mountain building were followed by glaciations but always after a lag comparable in length to that occurring between the Pliocene (" Alpine ") mountain building and the Quaternary glaciation. There is, as far as I know, no evidence that major changes in solar radiation took place just as these intervals. On the other hand, the lag can be explained purely by geographical factors without introducing extraterrestial causes.

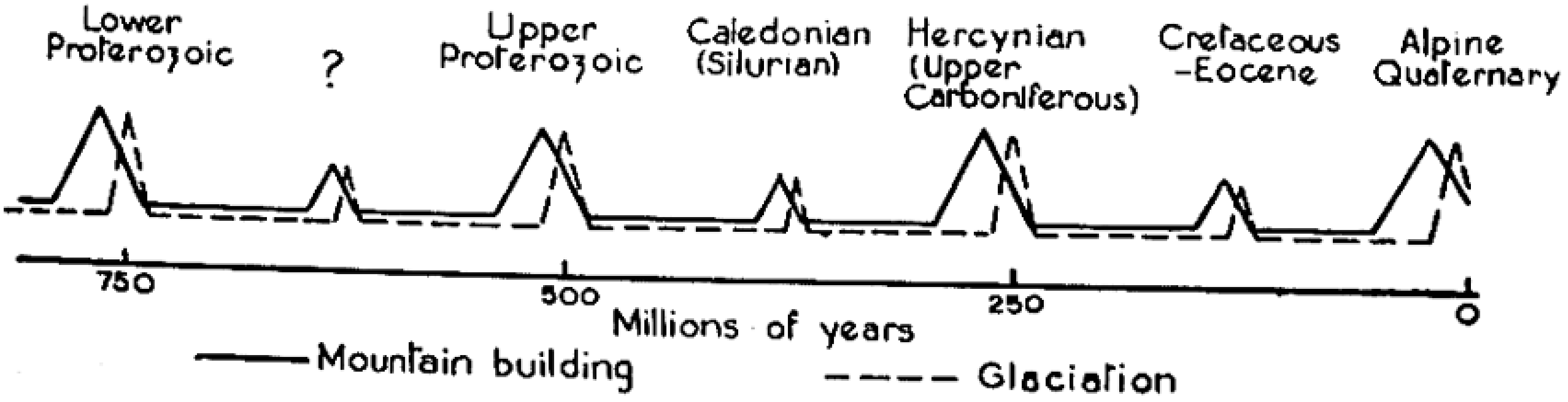

Figure 1. Mountain building and Glaciation (Schematic).
Reproduced from " *Climate through the Ages* " (revised 1949) by Dr. C. E. P. Brooks by permission of the author and Ernest Benn Ltd.

Two main conditions are necessary to promote and maintain an ice age : cold oceans and the formation of glaciers. At the present time, the oceans are cooled by water flowing equatorward along the ocean bed from the polar regions. This water comes to the surface in lower latitudes where off-shore winds drive the surface waters away from the coasts. The upwelling water appears as a cold current, one example of which is the Benguela current along the west coast of Southern Africa.

When the poles are unfrozen, the equatorward flow is much weakened and the water is not cold enough to lower appreciably the temperature of the oceans as a whole ; also the warmth of the oceans generally keeps the temperature at the poles above freezing point. It is not until piedmont glaciers, formed over land areas, reach the coasts and calve icebergs into the sea that sea temperatures become effectively lowered.

Glaciers do not appear immediately a spasm of mountain building is over. In the process known as mountain building, stratified portions of the earth's surface may either become uptilted to form smooth ranges or else be lifted vertically to form high horizontal plateaux. In either case, the high ground consists at first of comparatively smooth surfaces exposed to direct isolation and any snow forming is evaporated before it accumulates sufficiently to harden into glaciers. It is not until weathering occurs, and the ranges and plateaux are transformed to rugged peaks and mountain valleys, that snow accumulates and glaciers develop.

Four stages may be noted in the advance and retreat of the ice. Once an ice-sheet has formed, it tends to go on growing because it causes a decrease in temperature all round it, due primarily to radiation from its surface. Next, the increase in the amount of water existing as ice causes a fall in sea level and a raising of the effective level of mountain peaks ; and so orographic snowfall increases, helping to maintain the expanding ice-sheet. Owing to subsidence of the cold air in contact with the ice, however, storm tracks are displaced to the periphery of a large ice-sheet and, as it continues to grow in size, there comes a stage at which no snow falls at its centre and it starts to break up. The final stage is due to isostasy : the weight of snow and ice causes the land to subside, thus lowering the effective elevation above sea level and decreasing the chances of orographic snowfall. At the end of the Wurm glaciation (about 15,000 B.C.) Scandinavia continued to sink for another four to five thousand years after the ice had disappeared.

Another topic seems worthy of mention, namely periodicity in recent climatic records. Here also, the ideas are those of Dr. C. E. P. Brooks. It is well known that quasi-periodicities of the order of a number of years are neither regular nor, in general, persistent ; but there is a sense in which they do exist. Dr. Brooks has suggested that the periodicity exists not so much in the values of the meteorological element in question as in the probabilities of their occurrence. A year ago, he examined again a periodicity of about 52 years which he and Dr. J. Glasspoole had found in the general rainfall of England and Wales*. He divided the annual rainfall totals into three roughly equal groups : years at or near the maximum of the 52-year periodicity, years at or near minimum and years between. He then evaluated for each group the probability of a year with rainfall above average and the probability of a year with rainfall below average. At or near the maxima of the cycle, he found the chance of rainfall above average to be about twice that of rainfall below average ; whereas near the minima it was only about half that of rainfall below average. Several of the wettest years occurred near minima of the rainfall cycle.

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* Brooks, C. E. P. and Glasspoole, J., "British floods and droughts", 1928.

Dr. C. C. Wallén (*Swedish Meteorological Institute*) : I take this opportunity of bringing his best regards from Professor Ahlmann, who unfortunately is unable to attend this meeting, and at the same time bring to your notice some findings from my glaciological studies on the Kårsa Glacier in North Sweden.

As you all know, the climatic amelioration of recent decades was at first found to have occurred during the winter months and to some extent during spring and autumn. In his investigations around the North Atlantic coasts Ahlmann was able to show that the large glacier retreat which has occurred in that area has mainly been a result of the lengthening of the ablation season caused by increased temperature in spring and autumn. Precipitation and so accumulation conditions have been of secondary importance.

As far as the Kårsa Glacier is concerned I think I have been able to show that the variations in the ablation conditions are definitely of larger importance for its extension than the variations in accumulation. The relative importance of insolation on the one hand and of convection and condensation on the other in the ablation process are practically the same but what has caused the large retreat in late years is the absolute increase of the convection caused by the higher temperature while the insolation has remained constant.

The main reason for the increase of the convective term in the ablation equation has been the increase of the summer temperature that has occurred in North Sweden since about 1910. The lengthening of the ablation term has been of secondary importance in the retreat of the Kårsa Glacier. Everything indicates then, that the summer conditions are very important in the life of the glaciers in North Sweden and I believe also in all regions around the northernmost Atlantic. Mr. Liljequist has shown quite recently that a considerable increase in the summer temperature has occurred in Sweden, having started as early as 1910 in the northern parts but not until about 1920 in the southern parts. I think this mode of increase is worth attention from the point of view of changes in the general circulation.

Summarizing I may say that I believe it is worth while to pay a little more attention to variations in the summer circulation of the atmosphere than has been done so far.

I may add that most of the results mentioned here are published in my paper ; " The shrinkage of the Kårsa Glacier and its probable meteorological causes," *Geografiska Annaler*, 1949.

Mr. D. J. Schove : I have been investigating changes in " World Pressure Patterns " from 1851 to 1950. It is particularly interesting to trace the southward movement of the thirty-year period of maximum pressure, the P-max, in the North Atlantic (see Schove 1950, p. 147, Fig. 4). This occurred before *c. 1890* in Iceland and most of Norway, between *c. 1893* and *c. 1896* in most of Northern Europe but *c. 1917* in Portugal and the Azores and as late as *c. 1925* in Madeira. Preceding the P-max occurred the cold period which Professor Willett and Dr. Lysgaard have just shown to be conspicuous in temperate latitudes about 1885. This cold period is the T-min of my Fig. 5 which period had actually occurred *c. 1860's* in North Sweden, *c. 1870's* in Scotland, *c. 1880's* in England, *c. 1890's* in France and in the South Mediterranean had lasted into the twentieth century. Data for particular stations can now be found in Dr. Lysgaard's (1949) excellent work. The lack of consistent records make it impossible to determine whether the south-steering process affected the Pacific and the southern hemisphere, but Professor Willett's data indicate one way by which this principle can be tested.

Dr. Lysgaard's work makes it possible to show that south-steering occurs also in the individual months of January and July. Thus for January we have the following dates of the P-max :—

Scandinavia	*c. 1882 and 1883*
Iceland, Denmark	*c. 1886/89*
S. Greenland, Britain, Middle Europe	*c. 1892/1896*
Portugal, Madeira	*c. 1919/1921*

The cold Januarys occurred *c. 1860's* in Scotland and Scandinavia, near *c. 1890* in most of the rest of Europe, and *c. 1890's* in the Mediterranean. The T-min for January thus precedes the P-max for the same month, as has already been shown to be the case for the year as a whole.

The explanation of the south-steering of pressure anomaly patterns might lie in changes of ocean temperature. It is, however, interesting to note that sunspots undergo an analogous process of slow south-steering during the course of the 11-year cycle ; at the beginning of the cycle sunspots occur in high solar latitudes, near the end in low latitudes. The analogy must however be regarded as an analogy only, for sunspots do not really correspond with depressions. Nevertheless, in view of the 50-year cycle mentioned by Miss Carruthers it is tempting to note that wet periods associated with low decadal pressures (nearly 1 mb below long period normals) moved southwards across the British Isles in the 1870's and 1920's (see Schove, *op. cit.*, Table III, p. 43) and that it is quite true that in Britain the seventies and twenties of the last few centuries have usually been wet. Unlike most long cycles, the 50-year one does at least fit the facts, but this might be a coincidence, and the truth does not really seem to be so simple.

The Little Ice Age in mid-Europe was recently summarized in a table (Schove 1949). Documentary evidence indicates that both cold winters and hot summers became more frequent after 1540 ; the onset had therefore been associated with a supposed decline in circulation. I have since come across some direct evidence which indicates that after about 1550 SW. winds became less strong or less persistent ; this is based on the fact that sand dunes in SW. Wales ceased to move inland about that date and is summarized by Brooks 1949, (p. 312).

However, it would be rash to date the Little Ice Age on evidence from Mid-Europe only and it would be interesting to have a synthesis of Scandinavian evidence incorporating the glacial and biological studies of Professor Faegri. Moreover, I hope also to present to the society shortly the results of some Chinese and Japanese researches on the same period.

Tree-ring widths both from recently-felled and from old-building timber in North Scandinavia provide various series of index numbers which have been found to be related to temperature in the four summer months May to August. Detailed results (to be published elsewhere) show by empirical formulae that a temperature anomaly of 1°C in the "growing season" made a difference of about 0·1 mm to the tree-rings of North Scandinavia considered as a whole. It would seem that cold summers set in about the 1580's but that from 1655 to 1703, especially during the last twenty years of that period, summer temperatures were actually higher than in recent times. It is hoped that Mr. Lowther's work on English oak-dendrochronology will soon yield indices extending back 2,000 years. It would be interesting to know whether regions of quiet but rapid sedimentation existed near the mouths of rivers in lagoons or inland lakes whereby core-samples could be used to date post-glacial changes to supplement the admirable work of Mr. Ovey on the glacial period.

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Mr. H. H. LAMB (*Meteorological Office*) : The temperature anomaly patterns shown by Dr. Lysgaard for all the cold European winters (1929, 1940 and in less pronounced form 1924) which he has studied have the common feature of a considerable *warm* anomaly precisely in those regions which are normally the coldest on the earth, notably the Canadian Archipelago, Greenland and parts of the Arctic fringe of Asia. This pattern was repeated in very clear form in February 1947. The same distribution occurred during a cold spell in Europe early in March 1950, when a pocket of remarkably warm air was cut off from the circulation of lower latitudes and stagnated over the Canadian Archipelago. If the atmosphere had an excess of heat to dispose of, this flooding of the regions of strongest cooling with warm air would presumably be the most effective way of achieving it, at the same time cold air is swept over the warmer surfaces in middle latitudes.

Prof. Willett's data also suggest that during the 1920-40 period of the present climatic fluctuation

(i) there has been a net warming over the earth as a whole, presumably indicating more solar radiation received.

(ii) there has been most warming in the coldest regions of the northern hemisphere, effectively decreasing the overall temperature drop from equator to pole.

(iii) there has been a contrary effect in the higher latitudes of the southern hemisphere.

Flohn (1944), reasoning from the changes which normally occur between summer and winter, has pointed out that the southern hemisphere seems to react to increased thermal gradient (or increased supply of heat in the lower latitudes) by transporting more heat polewards in the disorderly manner accompanying the occlusion process in an intensified zonal circulation ; whereas the northern hemisphere tends to establish an orderly arrangement of warm, blocking highs in high latitudes and meridional airstreams. Willett however tells us that the changes in the northern hemisphere accompanying the sunspot cycle go this way towards the major sunspot maxima, but show an intensified zonal circulation instead about the minor sunspot maxima.

May we perhaps assume therefore that with increasing solar energy received both hemispheres at first respond alike, but that after the increase reaches a certain point the northern hemisphere tends to go over to a more or less orderly transport of heat in meridional airstreams with high-pressure systems developing in high latitudes ? Any such sudden change-over as that implied in this query and the critical conditions affecting it might be most easily detected in the individual situations and day-to-day play of the weather, since the anomaly patterns of a given year are a statistical result and in reality some blocking situations occur in most years.

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Flohn, H.	1944	*Gerlands Beitr. z. Geophysik*, **36**.

551.583.1 : 551.582.2 (65)

Professor PAUL QUENEY (*University of Paris*) : In a paper published 13 years ago (Queney 1937) I discussed the variation of precipitation in Algeria during the interval 1850-1937. It may be of some interest to give here a summary of this paper, and some brief comments about it.

(i) *Reduction of available data*

In Algeria, the network of meteorological stations was built up in two steps, respectively in 1875 and 1914-1920. The series of monthly precipitations are therefore very few and irregular in the first years of the French settlement (1838-1874), more numerous but still rather irregular from 1875 to 1914, then still more numerous (about 100) and quite satisfactory, except in the southern arid regions.

Because of the many gaps in the series prior to 1914, it is not possible to use them individually in order to know the secular variation of precipitation in Algeria but I thought it reasonable to get an idea of this variation by assuming that the secular changes are about the same in neighbouring stations belonging to the same climatic region, as justified by recent data.

For instance, in the coastal zone of the province of Oran (Fig. 1), the following stations may be considered as having experienced similar climatic changes : Nemours, Aïn-Temouchent, El-Ançor, Cap-Falcon, Oran, Mostaganem, Cap-Ivi. These seven stations never worked at the same time before 1914, but fortunately their working periods are largely overlapping, so that it is possible to reduce all their data to any one of the stations, by assuming their monthly precipitations to be proportional (that is, all the January rainfalls proportional, the same for the February rainfalls, *etc.*).

The same process was carried out for the nine climatic regions of Algeria shown in Fig. 1, these regions resulting in a twofold subdivision in zones parallel to the coast (L = coastal zone ; T = central zone of the Tell ; S = southern zone) and in perpendicular strips (O = province of Oran ; A = province of Algiers ; C = province of Constantine), denoted by the two corresponding letters (OL = coastal zone of the province of Oran, *etc.*).

This led me to a unique and continuous series of monthly values for each of these nine regions, covering the period 1850-1937 for most of them. Finally, in

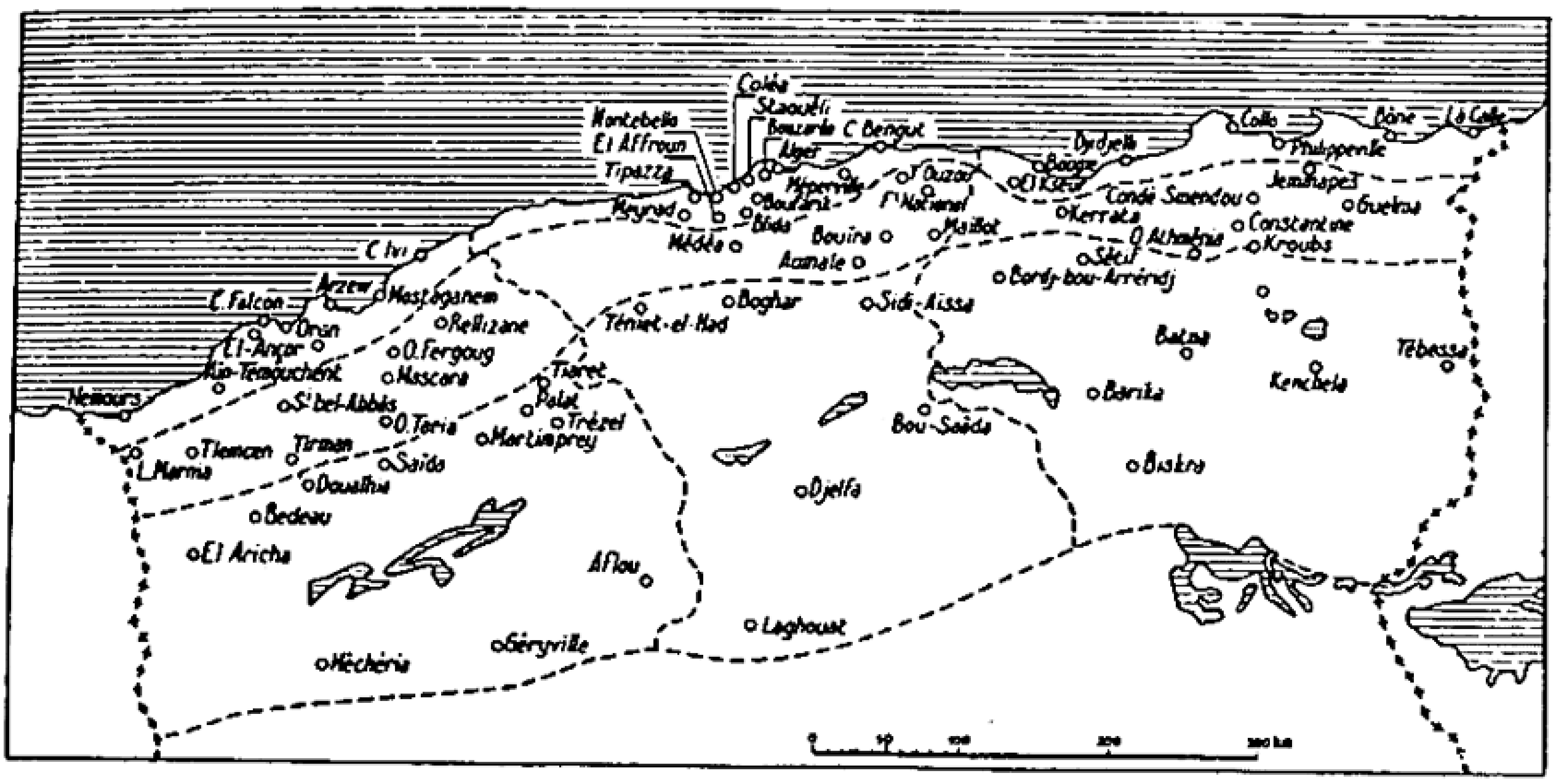

Figure 1. The nine climatic regions of Algeria, with the stations the data of which have been used.

order to compare these nine series, a further reduction was made by taking as a unit of precipitation, in each region, the mean annual rainfall for the period 1901-1930 (annual normal).

(ii) *Changes in the annual variation of precipitation*

In Fig. 2, the annual variation of precipitation of the nine regions of Algeria is given in the form of diagrams for three successive periods of 20, 30 and 30 years from 1850 to 1930. These diagrams show the permanence of a dry season in

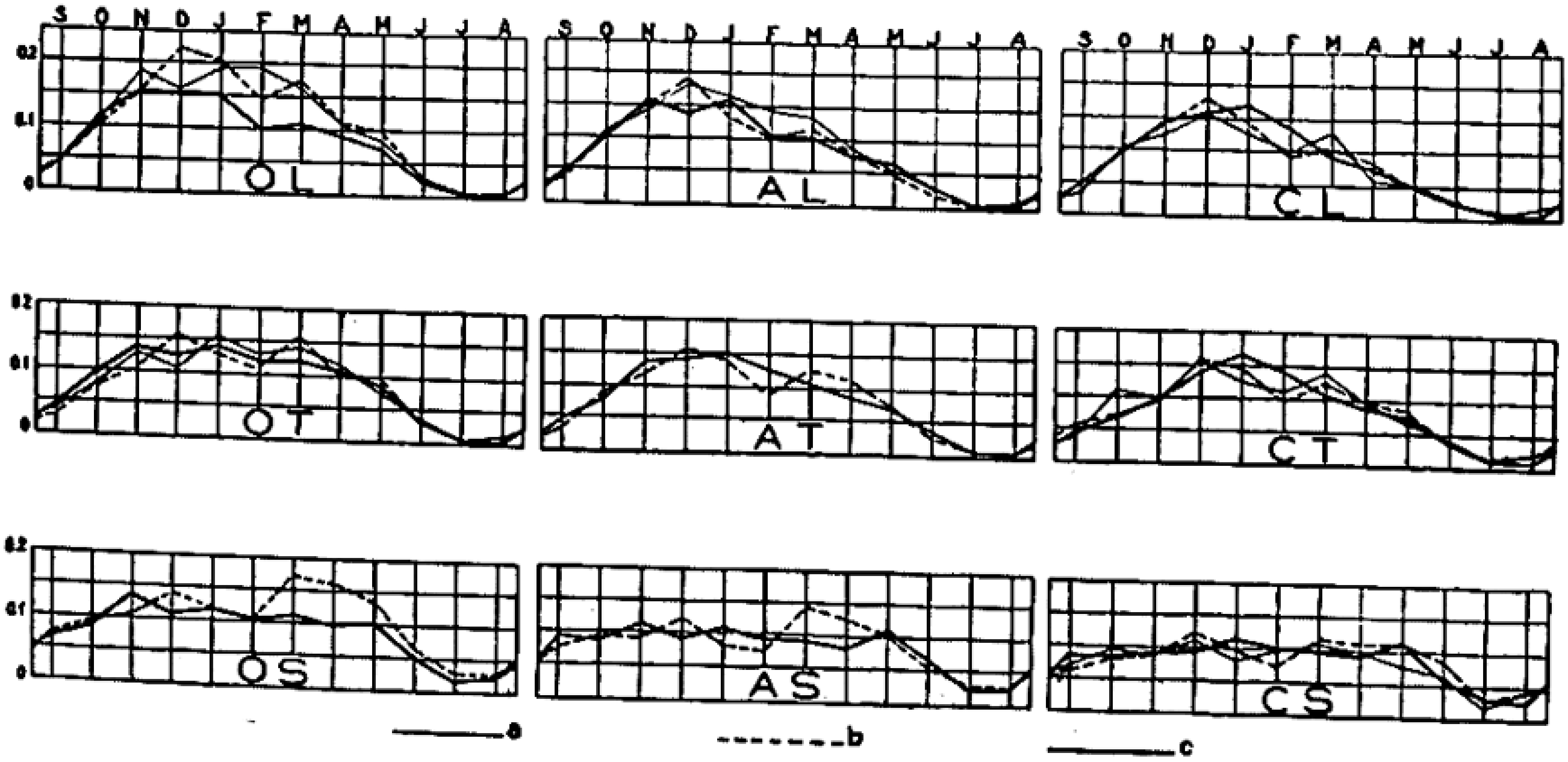

Figure 2. Mean annual variation of precipitation in the nine climatic regions of Algeria, during the three periods a (September 1850 — August 1870), b (September 1870 — August 1900) and c (September 1900 — August 1930). (In Algeria, the climatological year begins in September.) The unit used is the mean annual precipitation during the period 1901 — 1930.

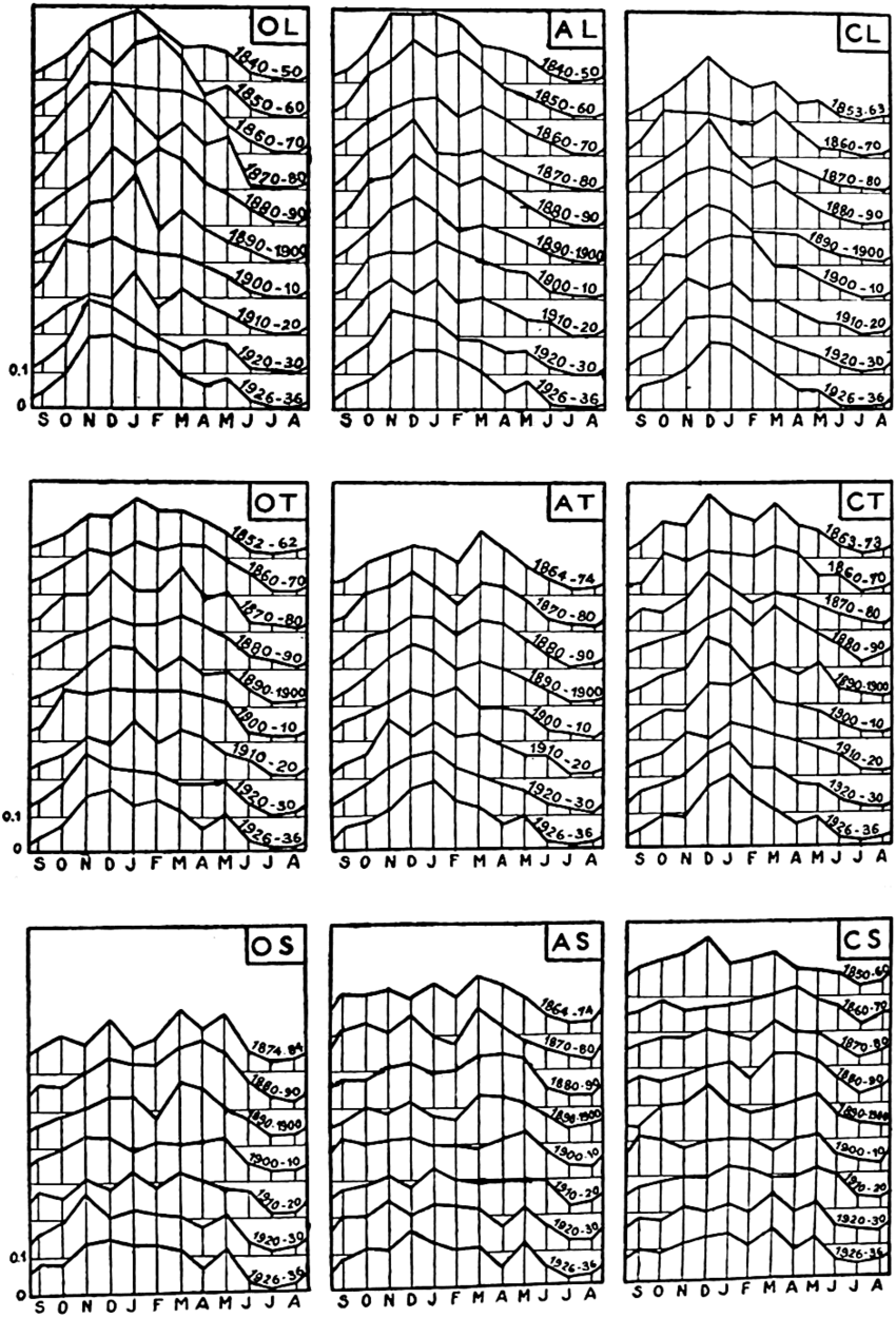

Figure 3. Mean annual variation of precipitation in the nine climatic regions of Algeria during successive 10-years periods beginning on 1 September. (For instance, "1840-1850" denotes the period "September 1840 — August 1850"). Same unit as in Fig. 2.

summer and a rainy one in winter, but some important and systematic changes appear when one proceeds from period b (1870-1900) to period c (1900-1930) :

(1) A general decrease in March in all the regions, and also in April except in region OT, this decrease being specially large in OS (38 per cent in April), AS and OL.

(2) A general decrease in December, but less important than in March, region OL excepted.

(3) An increase in January-February, except in OS where there is no appreciable change, and in OL where there is an important decrease.

From Fig. 3, giving the annual variation of precipitation by periods of ten years, it may be seen that in OS and AS the decrease in March occurred rather suddenly just at the end of the 19th century, so that one may speak of a real climatic change at that time.

(iii) *Changes in the annual amount of precipitation*

The above-mentioned increase of rainfall in mid-winter happens to balance almost exactly the decrease in March-April in the regions where it is observed, so that there is only a slight change in the annual precipitation from period b to period c, as shown in Table I. But in regions OS and OL there is no such compensation, and as a result an important decrease is observed in the annual precipitation (29 per cent in OL).

TABLE 1. Values of $(P_c-P_b)/P_c$ for the nine climatic regions of Algeria, where P_b and P_c are the mean annual precipitations for the 30 years-periods b (September 1870 — August 1900) and c (September 1900 — August 1930), respectively :

OL = −29 per cent	*AL* = − 2 per cent	*CL* = + 2 per cent
OT = 0 per cent	*AT* = 0 per cent	*CT* = − 2 per cent
OS = −21 per cent	*AS* = − 4 per cent	*CS* = + 1 per cent

We should mention here the severe agricultural consequence of the diminution of spring rainfall in regions OS and AS, a diminution which has been well recognised by all the farmers living there.

(iv) *Relation to dynamic meteorology*

The particular behaviour of precipitation in regions OL and OS may be related to the fact that rainfall in Algeria is mainly produced by three different kinds of atmospheric perturbations : the tropical perturbations, coming from south-west, with a normal path from OS to AL ; the polar-Mediterranean perturbations, formed on the Mediterranean Sea as a secondary effect of middle-latitude perturbations ; and the tropical-Mediterranean perturbations, formed on the same sea as a secondary effect of tropical perturbations. Because of the presence of the Atlas, the rainfall related to tropical perturbations mainly occurs in region OS, but the tropical-Mediterranean perturbations associated with them also give large amounts of precipitation in region OL. On the other hand, the polar-Mediterranean perturbations are more likely to bring rain into regions CL and AL, and also, though in smaller amounts, into regions CT, AT, CS and AS.

If we add that tropical perturbations are more frequent in March-April, and polar-Mediterranean ones in January-February, the above results concerning climatic changes in Algeria can be understood to some extent if the assumption is made that tropical perturbations generally decreased in frequency and intensity, while on the contrary polar-Mediterranean perturbations generally increased, at the end of the 19th century.

This result seems to be in good agreement with those of other authors, giving a general increase of precipitation in middle latitudes of the northern hemisphere, and a general decrease in low latitudes.

REFERENCE

Queney, P. 1937 *La Météorologie*, Paris, **6**, 1937, p. 429.

Professor FAEGRI (*in reply*) : I very much regret having to oppose Dr. Brooks's views considering he is not here, but I think that a word of warning should be said about assuming a too close connexion between orogenic phases and glaciations. After all, we can hardly rely on more than the last two glaciations. About the first we know very little indeed, and as for the Cambrian-Caledonian phase geological evidence suggests, if anything, that the relative importance of its two parts is the opposite of that proposed by Dr. Brooks.

At the same time I should like to endorse Dr. Brooks's views that we should not presume too radical changes of solar activity to explain Ice Ages. Martin Johnson has recently (*Science Progress*, 1949, **37**, p. 625) pointed out that as the heat generation of the sun is now ascribed to definite atomic processes, we cannot presume changes of this heat generation which are not warranted by the laws of atomic physics.

With regard to the changes of circulation patterns responsible for the anomalous winters of the early 1940's I want to point out that as they produce *different* effects in Arctic Canada and NW. Europe, they cannot be the same as those which have produced the *same* effects in both places during the Ice Age.

Finally I should like to point out that we have in those parts of Norway that are directly exposed to cyclonic activity, no traces of climatic deterioration *ca.* 1740 : on the contrary, our glaciers started recession from their last major advance *ca.* 1750. This represents another case of differential reaction.

Professor H. C. WILLETT (*in reply*) : As I understand Mr. Lamb's remarks, he has asked essentially two questions, one as to the basis of the difference between the general circulation changes from sunspot minimum to major sunspot maximum compared with those from a minimum to a minor sunspot maximum, and the other as to why both Dr. Lysgaard's winter temperature anomaly pattern severe cold in Europe and the very similar pattern for the cold winter 1946-47, are characterized by abnormal warmth in the normally very cold Canadian Archipelago.

With regard to the first question, no definite answer is possible. We can say that it is an observed statistical fact of the past 8 single or 4 double sunspot cycles that the major maximum is accompanied by a significant pressure rise north of 60° and the minor maximum by a corresponding rise between 45° and 60°. This difference has been found also by Clayton, and by Wexler from the Weather Bureau historical maps of the northern hemisphere. The difference can be attributed either merely to the different degree to which the solar disturbance develops in the two types of maximum, or to some organic difference in the physical nature of the solar radiations in the two cases, or merely to coincidence. The number of solar cycles has been too few, and our observational information too meagre, to choose reliably between these possibilities at present.

With regard to the warmth of the Canadian Archipelago at time of extreme coldness in western Europe, these two characteristics of the world temperature patterns constitute merely the opposite faces of the same coin. High pressure in the Greenland area, which normally characterizes extreme winter coldness in western Europe, establishes almost inevitably abnormally strong south-westerly flow of warm air from the North Atlantic Ocean into north-eastern North America, and the Canadian Archipelago. Hence the warmth of this region is caused by the warm current of the same pressure anomaly which produces the flow of cold air into western Europe. The temperature change pattern of the past twenty years, as analysed in my paper, does indicate a rise of temperature over north-western Europe, but it indicates a fall, not a rise, of temperature in the Canadian Archipelago.

Professor MANLEY: I am sure we all appreciate in the highest degree the importance of the work of Professor Ahlmann and his many colleagues in the field of climatology; and we are indeed sorry that Professor Ahlmann himself has been unable to come. We welcome Dr. Wallén who has recently published work on the glaciers of Northern Sweden. The whole subject of climatic change has received a stimulus, as we can now recognise that we are living in a minor fluctuation whose dimensions can be measured and the effects observed. We are beginning to be able to compare the order of size of these minor fluctuations with those of the post-glacial age with which we are now acquainted. Professor Faegri has done a great service to meteorologists in reminding them of the pitfalls associated with the botanical evidence on which we so largely depend. Yet I think there is evidence (*cf.*, *Q.J.* **75**, p. 170, 1949) that the order of size of the climatic fluctuation in North-West Europe represented by the Allerød Oscillation is similar to that of present day fluctuations between the periods of greater and less 'vigour of the circulation,' however caused. Through the connexion between the 250 years of instrumental records, and the fluctuations of mountain glaciers, climatologists have now the opportunity to use the present as the key to the past.

I can offer some illustrations (which will appear in a forthcoming paper in the *Geogr. J.*) of the extent of the present minor amelioration in England which has become appreciable since about 1925. With regard to 'accumulated temperature' during the growing season it compares with other minor fluctuations during the past 200 years. If this slight but appreciable amelioration were in time to undergo a sudden and lasting recession within the range of values represented during the past two centuries the effects might well be similar in magnitude to those of the 'climatic recessions' known from botanical evidence to have occurred in the post-glacial. In other words the swings in the past were not seriously greater in magnitude, but they might have been of longer duration, than those to which we are accustomed. It remains to be seen whether we can now provide a climatic interpretation which will satisfy not a part, but the whole, of that botanical evidence for which we must go to Professor Faegri and his colleagues.

Professor Willett's conclusion that the prevailing rise of temperature on the whole began about 1885 carries much conviction and his analysis of this rise in relation to global position is extremely stimulating in the light of his conclusions with regard to the intensity and location of the winter 'boreal' anticyclones, a title I think we might adopt with some advantage. Much of his argument however depends on the trend revealed by certain higher-latitude stations and these are as yet few in the southern hemisphere. I do wonder if some of the long-term series quoted by Clayton have been sufficiently standardised to be acceptable when we are tackling these very small overall changes over many years. Moreover in places such as West Greenland, small changes in the extent of the sea-ice may give rise to large local temperature effects. No doubt in time we shall know which records are most useful and significant in estimating the magnitude of these trends, and we shall soon know a good deal more about the Antarctic. We are most grateful to Professor Willett for a stimulating reminder that we should again pay attention to the 'Boreal Highs.'

Dr. Lysgaard's independent support for Professor Willett's views with regard to the tendency for intensification of the boreal anticyclones is noteworthy, likewise his findings with regard to the development of what Sir Gilbert Walker used to call the North Atlantic Oscillation. Dr. Lysgaard has provided a tremendous assemblage of data and the extensive series of charts which he has shown us, which are only a few of those published in his recent exhaustive paper (*Folia Geographica Danica*, Tom. V, Copenhagen, 1949) will indeed provoke much thought.

I think it can be shown that even in glacial times the climatic recessions and ameliorations represented by the standstills and retreats of the ice are of broadly similar dimensions to those of the post-glacial age. In this I agree with Professor Willett in principle, but I am inclined to differ from him on the question of magnitude, a point I have tried to develop in a paper in course of publication. The important point emerges, I think, that if the range of climatic fluctuation with a cold icy Atlantic is broadly comparable with that accompanying a warm Atlantic, we may well need to reconsider some of our ideas of the cause of variations in the vigour of the general circulation.

We can welcome Mr. Schove's contribution which reminds us that in historic records there is still room for enlightenment regarding the onset of the 'Little ice age.' We need to remember the very wide scope of climatological enquiry and the need to take account of many possible clues. Captain McClean's papers which make use of the aggregate diagram in the study of fluctuations of rainfall in Scotland, will be appreciated by many workers in that field. He has placed on the table the evidence for a somewhat extraordinary agreement between the long-term diagram for Auckland (New Zealand) and those for the Thames basin. We should not lose sight of the possible clues to the long-term world variations which may be brought to light by such work. Many years earlier Sir Gilbert Walker, whom I am indeed sorry not to see present, investigated many correlations between the meteorological events at widely separated places, and, as Professor Willett and Dr.

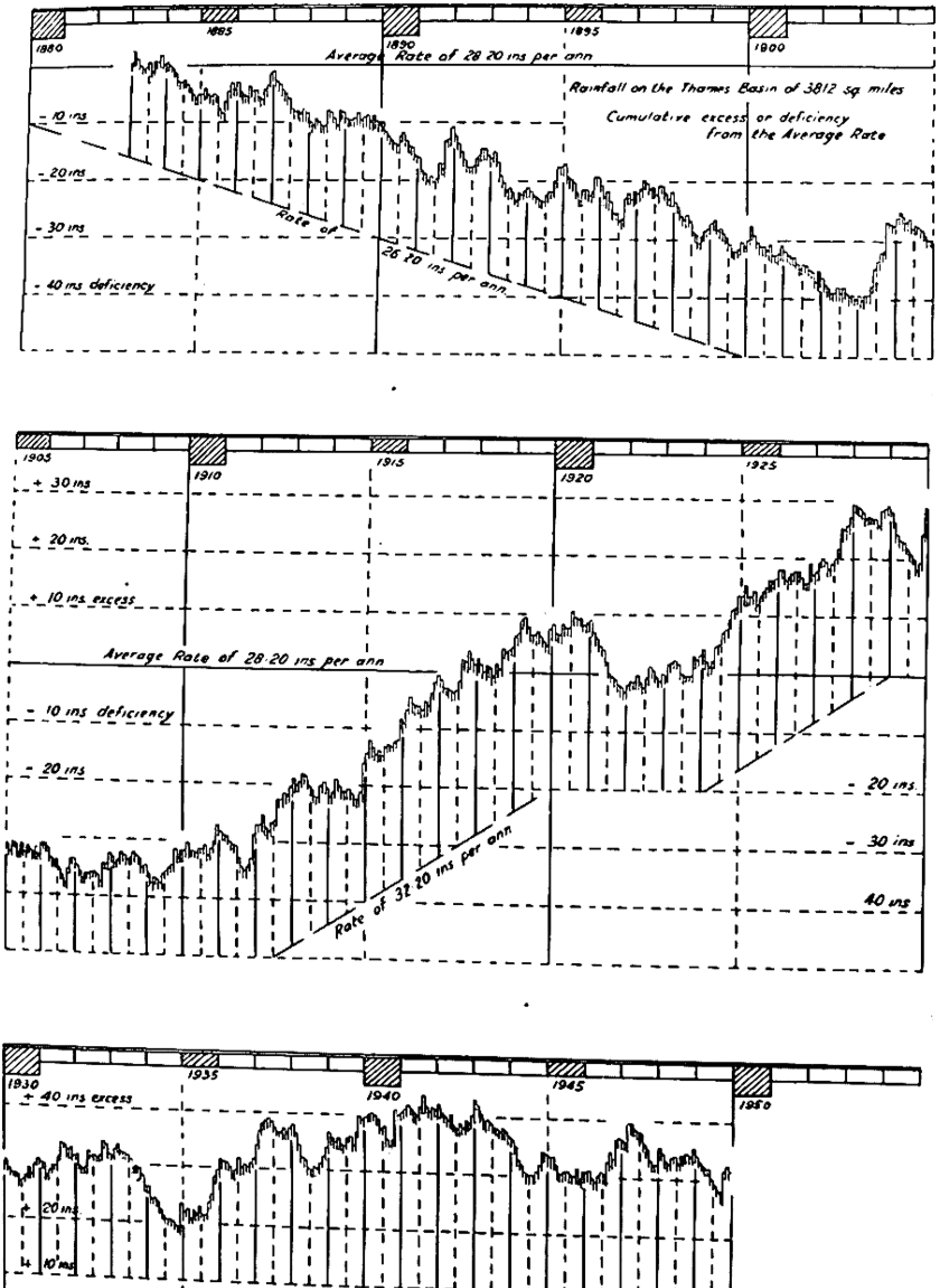

Figure 1. Rainfall over the Thames Basin 1880-1950, as cumulative excess (or deficiency) from the average for the period 1883-1917.

Lysgaard have shown in their own papers, the world-wide comparison is still a necessary and indeed fruitful method. The fascination of the study of climatic change has not only an economic significance for all of us who live towards the outer margins of the temperate zone ; it may well have a deeper appeal on account of the fact that we as men owe our evolution so largely to the climatic stresses with which our ancestors strove. Perhaps we are still striving towards adaptation, as many members of the audience will no doubt agree after living in a fireless room during this cool, easterly weather of the Oxford spring.

Captain W. N. McClean (*communicated*) : The graphical record given in Fig. 1 for the rainfall on the Thames Basin of 3,812 mi^2 is compiled from the statistics of the Thames Conservancy, by their kind permission. The monthly rainfall on the area is assessed from the records of 12 gauges, suitably distributed over the area.

The standard 35 years' average (in this case 1883 to 1917) is 28·20 in. *per annum*. This average rate is represented on the graph by the full-black horizontal line ; and the actual aggregate rainfall, from the start up to any date, is shown as an excess or deficiency from the aggregate of the average rate. Thus, the rate of rainfall, over any intermediate period, is given by the slope of the graph, as so many inches of excess or deficiency from the horizontal average, in the number of months constituting the period.

The general trend of the graph shows the long dry period before the exceptionally wet year of 1903, with 2 in. less *p.a.* than the standard average. From 1910 to 1920, the average annual rainfall is about 4 in. *p.a.* in excess of the standard average ; and, excluding the critical droughts of 1921 and 1929, this wetter average continues up to 1930. During the final 20 years, the wetter average has been gradually broken down, until there appears to be a tendency to return to the drier average of the early years of the record.

Many other rainfall records in the British Isles show, more or less distinctly, rather similar changes in long-period average ; but shorter periods may be very different. Actually there appear to be somewhat similar trends at Auckland in New Zealand. It would be interesting to know how far these general trends are similar or dissimilar in different parts of the British Isles, and elsewhere.

CENTENARY SYMPOSIUM
ON
METEOROLOGY AND THE COMMUNITY

Held at Oxford on the morning of 31 March 1950

Chairman : SIR ROBERT WATSON-WATT, F.R.S.

551.5 : 06

Services rendered to the community by the Meteorological Office

By SIR NELSON JOHNSON
Director, Meteorological Office, London

1. INTRODUCTION

It is proposed to give an outline of the services rendered to the community by the British Meteorological Office, and to indicate the ways in which it is hoped to improve those services in the near future.

As a matter of historical interest it may be noted that the Meteorological Office was formed in 1854 as a Department of the Board of Trade with the object of providing the British merchant navy with information about the winds over the oceans on the trading routes. Since that time the Meteorological Office has developed into a State meteorological service responsible for meeting the needs of the community generally, although the Marine Branch still remains an important part of the organisation.

2. PRESENT USERS

The various sections of the community which the Meteorological Office serves today are given in the following list :—

- (a) General public
- (b) Shipping and fishing fleets
- (c) Defence Forces — (Navy)
  - Army
  - Air Force
- (d) Civil Aviation
- (e) Public utility services
- (f) Agriculture
- (g) Industry
- (h) Town Planning

Responsibility for giving meteorological advice to the Admiralty and H.M. Ships rests with the Naval Weather Service which, however, utilises basic information supplied by the Meteorological Office.

The nature of the services rendered to the various sections of the community is outlined in the following paragraphs. Further details will be found in a small book bearing the title *Your Weather Service* which is to be published almost immediately by His Majesty's Stationery Office.

3. GENERAL PUBLIC

The general public is served in five main ways, *viz.* through the B.B.C., the Press, by direct personal advice, by means of official publications and by correspondence.

Weather forecasts and warnings are issued through the B.B.C. (Home Service) every day at 0655 (except on Sundays), 0755, 1255 and 1755 clock time. The messages contain, as appropriate, warnings of gales, snow or frost, a general weather forecast, further outlook, and district and sea area forecasts. In these forecasts the land areas are divided into 20 districts as shown in Fig. 1.

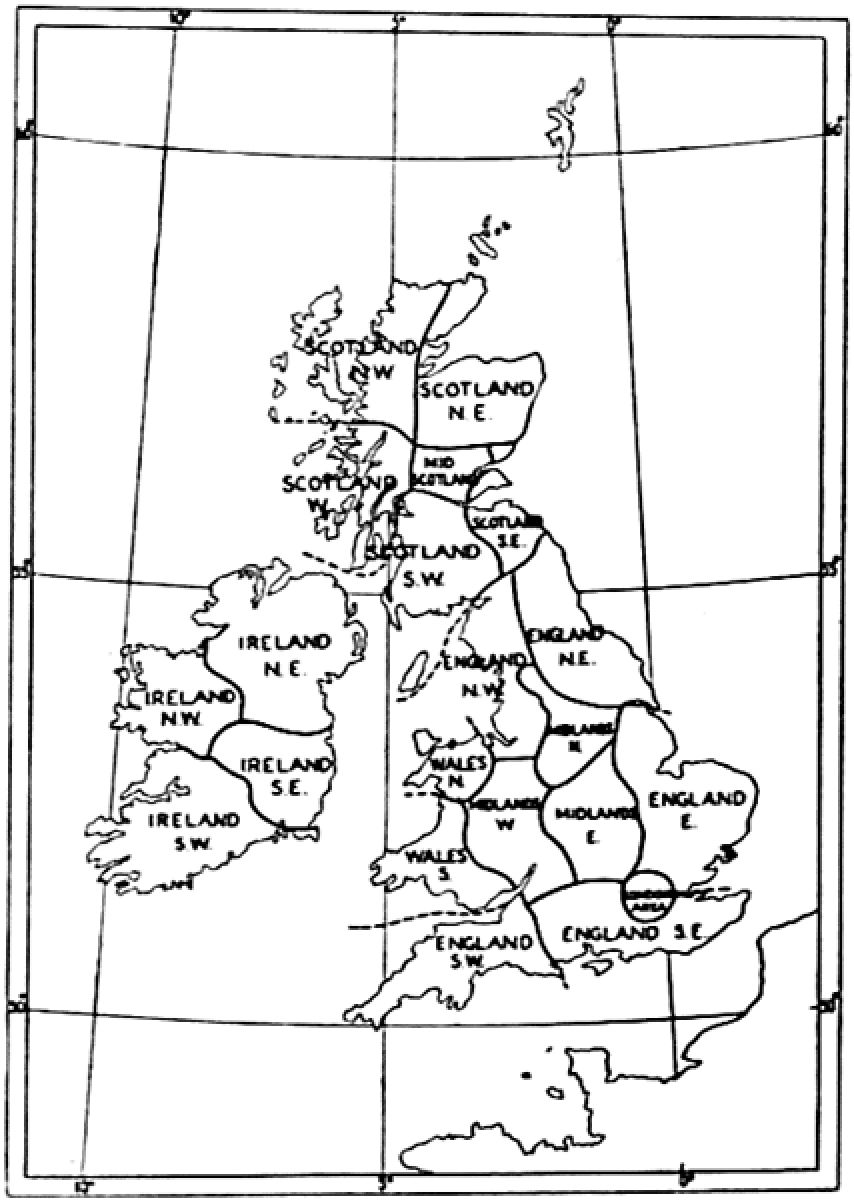

Figure 1. Forecast districts.

In addition, a meteorological item is included in the B.B.C. Television Service at about 10.30 p.m. each evening. Two charts are shown, the first of which depicts the present weather situation in semi-diagrammatic form, and the second shows the corresponding configuration of pressure systems and the weather expected over the country at 8 a.m. the following day. While the first chart is being shown, a verbal description is given of the present situation, and the forecast is announced while the second chart is being displayed.

Forecasts are supplied to the Press nine times a day, the most important being that issued at 2100, which is accompanied by two synoptic charts, one showing the present situation and the other the forecast (prebaratic) chart for noon the following day. Owing to limitation of space, no newspapers are able to publish both charts. Those newspapers which are able to find space for one chart print the second chart, which corresponds with the mid-epoch of the forecast issued at the same time.

The public are able to consult a meteorologist and obtain forecasts and personal advice at the London Headquarters of the Meteorological Office in Kingsway, and at the 16 provincial meteorological offices shown in Table I given below.

TABLE I. List of London and provincial meteorological offices from which forecasts and information can be obtained

Place	Telephone No.
London	Holborn 3434. Ext. 629
Abingdon (Oxfordshire)	Abingdon 288. Ext. 121, 209
Aldergrove (N. Ireland)	Antrim 2202. Ext. 125/6/7
Bawtry (Lincolnshire)	Bawtry 363 - 7
Dyce (Aberdeen)	Dyce 33/2. Ext. 70
Eastleigh (Southampton)	Eastleigh 87228/9. Ext. 10
Elmdon (Birmingham)	Sheldon 2441/5. Ext. 15, 24
Gloucester	Gloucester 24465/6/7. Ext. 110/1/2
Mildenhall (Suffolk)	Newmarket 3151. Ext. 16, 17 Mildenhall 2282-2
Pitreavie (Fifeshire)	Inverkeithing 264-8. Ext. 118/9
Plymouth	Plymstock 2224. Ext. 108/9
Preston (Lancs.)	Preston 4602. Ext. 200
Renfrew (Dumbartonshire)	Renfrew 2352. Ext. 21, 23
Shawbury (Salop)	Shawbury 351
Speke (Liverpool)	Garston 1240. Ext. 21, 22 Garston 2437.
Upavon (Wilts.)	Upavon 7-8. Ext. 8, 9
Watnall (Notts.)	Nottingham 45731. Ext. 230/1.

4. Shipping

The needs of shipping are provided for in the four B.B.C. broadcasts referred to in the previous paragraph. For this purpose the region in the vicinity of the British Isles is divided into the 24 sea areas shown in Fig. 2.

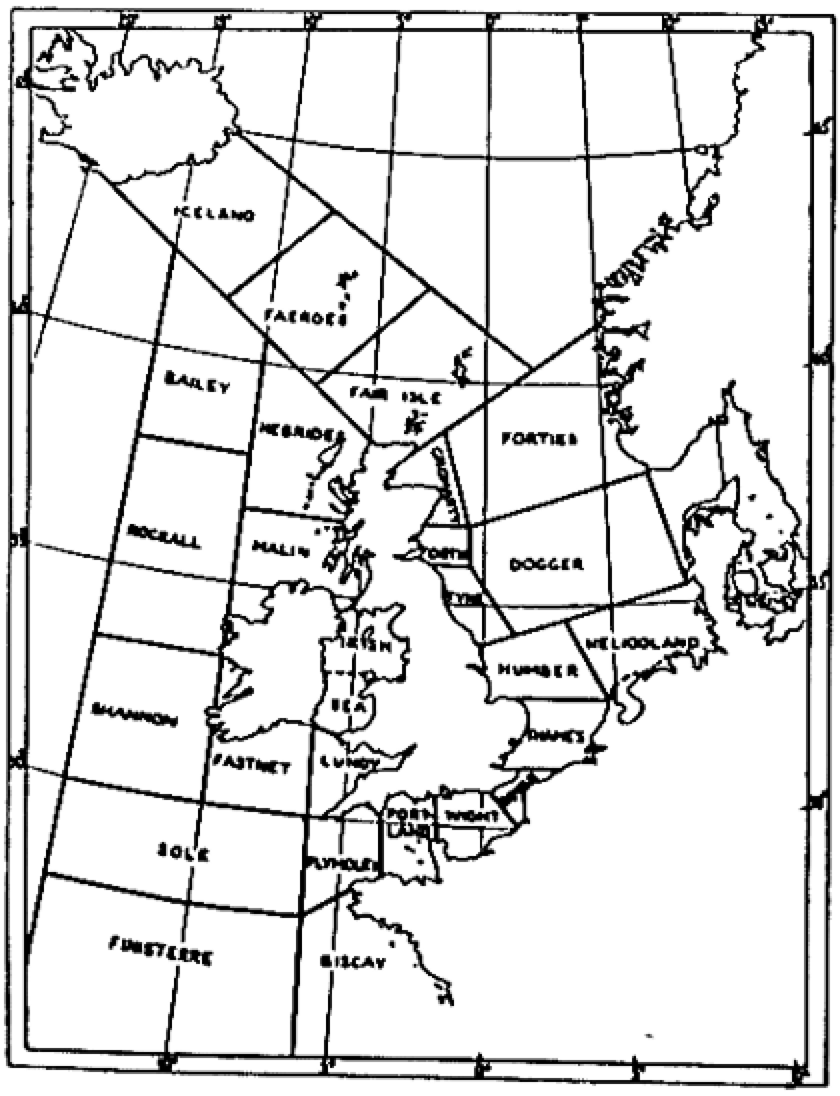

Figure 2. Coastal sea areas used in forecasts for shipping and gale warnings.

For the benefit of coastal shipping, weather bulletins are issued from nine W/T stations and from 11 R/T stations each morning and evening. An Atlantic weather bulletin is broadcast from Portishead by W/T at 0930 and 2130 each day. Details of these transmissions (call signs, times, frequencies) will be found in the *Handbook of Weather Messages, Codes and Specifications — Part I. Transmission Schedules and Station Index Numbers*, M.O. 510 (a), published by H.M.S.O.

Gale warnings are issued immediately by the B.B.C. in the Light Programme (1500 m) and by the W/T and R/T stations referred to above. Gale warnings are also sent to Coastguard stations and similar coastal positions at which Storm Cones are displayed.

5. DEFENCE FORCES

In time of peace the main requirements of the Army are at experimental establishments and gunnery schools.

On the other hand, the R.A.F. requires meteorological advice in connection with the day-to-day flying carried out at a large number of Service airfields throughout the country.

At each Command Headquarters a meteorological adviser is provided to make a study of the problems peculiar to each Command. It is his duty to look ahead and see what meteorological problems are likely to arise as a result of the trend of developments in Service aircraft and their tactical employment. Steps must then be taken to obtain solutions to these problems by the time they are required operationally.

6. CIVIL AVIATION

The primary requirement of civil aviation is the provision of route forecasts at the main terminals such as London Airport and Prestwick. Route forecasts are supplied to every aircraft which leaves the airport. For long distance flights the forecast has to be " zoned " in such a way that the forecast for each " zone " corresponds to the time that the aircraft will be in that particular zone. The route forecast therefore takes the form of a staggered or " composite " forecast. The captain of every aircraft is carefully briefed and supplied with a detailed forecast for his flight. This includes charts for sea level and for the 700 mb and 500 mb levels, together with vertical sections showing the cloud and weather conditions to be expected along the whole of the route. Forecasts are also given for the destination airport and " alternates " in case of diversion.

The number of aircraft now in operation makes it necessary to keep them under careful control. For this purpose there are five Air Traffic Control Centres at the following places :—Uxbridge, Gloucester, Preston, Prestwick, Inverness. Each of these is assigned an area within which it is responsible for the safety of all aircraft in flight. For this purpose each A.T.C.C. is provided with a meteorological office to keep the Controller continuously informed of the actual and expected weather within the region, and in adjoining regions.

In order to reduce as much as possible the amount of meteorological information to be supplied to individual aircraft, the four southernmost A.T.C.Cs. issue every half hour a " sub-area broadcast," giving a report of the present weather and forecasts for certain key aerodromes in the region.

As in the case of the R.A.F. Commands referred to in the previous paragraph, so the British Air Corporations find a need for a meteorological adviser to study their special problems from the operator's point of view. When a problem has been carefully analysed and formulated, it can then be referred to the Meteorological Office for the Special Investigations Division to study and provide the answer to it.

7. PUBLIC UTILITY SERVICES

Among the public utility services which are most affected by the weather naturally come the transport services. In particular, British Railways find it valuable to receive warnings of fog, snow, frost, gales and glazed frost. Such warnings are issued throughout the winter months from 12 Meteorological Office out-stations to 46 Railway Control Centres in various parts of the country.

Similarly, road transport needs warnings of snow and fog. These are supplied to Headquarters of the Royal Automobile Club and to 15 County Offices of that Organisation.

The British Electricity Authority is provided with a forecasting unit at its headquarters to provide constant information on the trend of the weather, which is liable to have important effects on the load which the Authority is called upon to provide. Temperature is naturally the most important element, but other elements, especially rain and cloud, have a marked influence upon the load. Apart from the London Headquarters, seven Grid Control Centres also receive reports and forecasts five times a day from six provincial meteorological offices.

Gas supply undertakings also need warnings of warm and cold weather, of frost, snow and fog. Such warnings are issued to 58 Gas Companies from 13 meteorological offices in various parts of the country.

Throughout the winter a special series of week-end forecasts is supplied to the Ministry of Fuel and Power and to 13 Regional Headquarters, to assist them in planning their week-end arrangements, and in particular, in deciding how much plant can be shut down for overhaul and maintenance work.

Local Authorities in the persons of County Surveyors, City Surveyors and Divisional Road Engineers, are supplied with warnings when snow is expected, to enable them to plan snow-clearing arrangements. Such warnings are issued to 44 Local Authorities from six Meteorological Office outstations.

8. AGRICULTURE

Although the Meteorological Office has a Branch whose sole responsibility it is to deal with agricultural meteorology, it is believed that more can be done for agriculture than is at present being achieved. For the last two years a senior meteorological officer has been attached to the National Agricultural Advisory Service Headquarters at Bristol. His primary duty has been to ascertain by contact with the agricultural fraternity what their particular problems are, and how the meteorologist can help to solve them. A beginning has been made in this way, and it is hoped to extend it. The problem of providing forecasts which are adapted to the requirements of farmers is also being studied. A difficulty which presents itself arises from the fact that no ready-made system of communication exists for distributing forecasts to farmers as is to be found among most of the public utility services already mentioned. Special forecasts for farmers are issued at harvest time.

9. HYDROLOGY

Working in collaboration with the Thames Conservancy, the Meteorological Office is able to issue to the Home Office authorities warnings when flood conditions are liable to occur and cause the Thames to overflow its banks. Standing arrangements provide for this service throughout the winter months.

It has become increasingly clear during recent years that water is likely to become a factor of great economic importance to the country before long. Thanks to the excellent organisation which was handed over to the Meteorological Office by the British Rainfall Organization, detailed information is available of the precipitation which falls on all parts of the country. Much, however, remains to be learnt about run-off, percolation and evaporation. This knowledge is of importance not only in connection with water supplies and the siting of hydro-electric projects, but also, of course, in connection with agriculture. The Meteorological Office is accordingly giving increasing attention to the whole question, and is making a special study of hydrology in collaboration with other interested departments and establishments.

10. INDUSTRY

As in most other applications of meteorology, the help that can be given to industry may take the form of advice in connection with planning, or the supplying of current forecasts. As an illustration of the former, the Meteorological Office is assisting in the design of the new Severn Bridge by carrying out investigations into the wind gustiness that must be expected at the site of the new bridge. For this purpose, information is required not only of the longitudinal gustiness of the wind and its fluctuations of direction, but also of the vertical component of the wind gustiness which is liable to produce large and unexpected forces upon the roadway and other horizontal surfaces of a bridge. Special instruments have been designed for the purpose, and will be mounted at a number of positions to provide this fundamental information.

As an illustration of the provision of forecasts to industry, reference may be made to the arrangements which exist for supplying forecasts to film producers in the London area. These forecasts may take the form of routine forecasts, giving the conditions expected on the following day, or they may be notifications when specified conditions, *e.g.* snow, fog, *etc.*, are expected to occur in any specified part of the British Isles.

11. TOWN PLANNING

The Meteorological Office provides the Ministry of Town and Country Planning with information relating to the proposed sites for new towns.

The Meteorological Office has had under preparation for some years an *Atlas of Climatology of the British Isles*, which is now being printed, and which, it is hoped, will be published in the near future. This atlas contains nearly 100 maps depicting the distribution of all the more important meteorological elements month by month. It is believed that the information contained in this atlas will find application in many directions.

12. THE FUTURE

Looking towards the future, the Meteorological Office hopes to promote a greater use of meteorology in industry, and especially in agriculture. It also aims at developing hydrology in view of its economic importance. While endeavouring to show the public in what ways meteorology can be made to serve their ends, the Meteorological Office is doing all it can internally to improve the information it is able to supply to the public.

551.5 : 621.3, 551.5 : 69

Meteorology and engineering

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1. INTRODUCTION

Meteorology is regarded by some as a purely academic study, by others as merely a means of forecasting tomorrow's weather. I would put forward a wider view, that meteorology is a part of human culture, which has applications in almost all aspects of life and work. Much of what I have to say is mainly concerned with getting forecasts of the right type to the right people at the right time, but the application of meteorological modes of thinking to engineering problems is a service we can render to the engineer, without the assistance of any large organisation.

I propose to deal rather briefly with some of the aspects of engineering in which the meteorologist can be of assistance to the engineer. In the time at my disposal it is not possible to do more than give some indication of the nature which this assistance can take in a few typical cases.

2. THE GENERATION AND TRANSMISSION OF ELECTRICITY

(a) *The location of a power station*

It may be accepted that the centre of a built-up area is not a desirable location for a large power station. There are, however, other sites which are equally undesirable, when considered from a meteorological point of view. For example, a long narrow valley with grassy slopes on one or both sides, from which the cold air formed near the ground flows down to the floor of the valley, will on a clear night be filled by a deep layer of cold air, with a pronounced inversion of temperature up to a height of some hundreds of feet above the ground. Within the inversion layer the emissions from the chimney will tend to accumulate, and since the chimney emissions contain both dust and sulphur dioxide, there will develop conditions which are unpleasant to human beings.

Even on flat ground, the chimney emissions can be unpleasant, both as regards dust and sulphur dioxide, unless certain precautions are taken. These precautions are discussed below.

(b) *The design of a power station*

The maximum concentration of sulphur dioxide at the surface, due to the emissions from a chimney, is inversely proportional to the square of the height of the top of the chimney above the ground. It is thus advantageous to build the chimney to the maximum practicable height. Further, if the emissions from the chimney are to be carried away downwind without being carried down to the ground by the wind, the height of the chimney above the ground must be not less than 2½ times the height of the roof of the building above the ground. This rule was laid down by the Electricity Commissioners about 1932, on a suggestion which I made to the Committee which was set up at that time by the Commissioners, and it has been found that, when a power station is built on level ground, this rule is generally sufficient to ensure that chimney emissions shall not be brought down to the ground in the lee of the building.

The fine dust which is emitted with the fumes diffuses much as do the gaseous constituents of the fumes, and the maximum surface concentration of dust is also inversely proportional to the square of the height of the chimney. Even when the best modern grit and dust eliminators are used, there will still be emitted a quantity of dust which will attain a considerable total in the course of a year. It is therefore desirable to minimise the nuisance which can arise from the dust by building chimneys of sufficient height.

In winds over about ten miles per hour, the concentrations of all chimney emissions are inversely proportional to the wind speed. In winds of lower speeds, there is a tendency for the emissions to rise vertically from the top of the chimney. This is in effect equivalent to increasing the height of the chimney.

(c) *Natural-draught cooling towers*

The condensers of the steam plant at a power station are water-cooled, and unless there is a large supply of water available, it is necessary to re-cool the thus heated water so that it can be used again. Such cooling is carried out by means of cooling towers.

The essential process within a cooling tower is the evaporation of a portion of the warm water, the latent heat required for the evaporation being taken from the bulk of the water. The air which comes into contact with the warm water is thereby rendered more humid, and may be also warmed. It will become less dense than the ambient air, so that there will be a fall of pressure at the base of the tower. In consequence, there is a pressure gradient producing a flow of air into, and up through, the tower. A cooling tower will not work in all conditions and it is a problem for the meteorologist to decide whether a natural-draught cooling tower will work in any specified conditions.

Some cooling towers in this country yield rather copious rainfall in their immediate neighbourhood, within distances of a few hundred yards downwind. To the meteorologist, it is a simple exercise to show that such rainfall is not due to condensation, as used to be firmly believed, but is due to the carry-over of unevaporated drops by the updraught through the tower. It can readily be shown that no drop of water formed within the tower remains within the system long enough to reach a sufficient size for it to fall to the ground within such short distances from the tower.

While the rainfall near the tower, mentioned above, is a mechanical rather than a meteorological phenomenon, the tower can produce other effects which are more truly meteorological. When the ambient air is cooler than the water which is led into the cooling tower, the air which leaves the top of the tower is warmer and more humid than the ambient air. It will also normally contain small drops of water in suspension, formed by the mixing of part of the incoming air with air which has come into contact with the warm water. During this process of mixing there will normally be condensation of water vapour into small water drops, which are carried out through the top of the tower along with the humid air, which mixes with the air outside as it is carried away downwind. Thus, even when the tower does not produce heavy precipitation in its immediate neighbourhood, it produces cloudy air which, when brought into contact with cold ground, will deposit liquid water or ice, according to the surface temperature, on the ground. Such ice will be in the form of a smooth sheet, capable of being a danger to traffic.

The water which is lost by evaporation within the tower must be replaced from some other source, and this so-called ' make-up ' will amount to 0·2 to 0·3 million gallons per day. Thus the question of location of power stations is closely bound up with water supply, and in general this means that it is closely bound up with the rate of flow in rivers. On this last point information is seldom, if ever, adequate, and it is most desirable that a thorough investigation of river flow should be made in this country. It should be remembered that water in large quantities is not plentiful in England, while in Wales, apart from the rivers Severn and Wye, the rivers are most inadequate for the needs of power stations.

It may appear that a disproportionate amount of attention is here given to the cooling tower. In the other matters discussed here, the function of the meteorologist is to supply data and forecasts at the right time and to the right person. But for the cooling tower the requirement is not a supply of data or of forecasts, but the application of arguments familiar to the meteorologist to a new problem of condensation.

(d) *The transmission of electricity*

The effects of meteorological phenomena on the transmissions of electricity were discussed at a joint meeting of the Institution of Electrical Engineers and the Royal Meteorological Society on 18 October 1945, and so do not call for detailed discussion here. Overhead lines may be broken down by the combination of wind and ice loading, while lightning may cause breakdown of insulation. Snow may deposit on conductors during snowfalls, and a subsequent lowering of temperature may freeze the snow, causing it to adhere to the conductor.

The meteorologist can help the engineer by supplying statistics of the frequency of occurrence of these phenomena, and by providing reliable forecasts of their probable occurrence in the immediate future. Such forecasts need not be made long in advance, but they must be of the highest accuracy if they are to serve the engineer effectively.

In connection with the variation of load due to variations of weather leading to suddenly increased demand for current for either lighting or space heating, such as occurred in the early months of 1947, long-period forecasts of the likelihood of severe weather over a long period would be of value to the engineer, whether he be concerned with the generation or with the transmission of current.

3. CIVIL AND MECHANICAL ENGINEERING

(a) *Wind pressure on structures*

One of the most obvious engineering problems which is bound up with meteorological factors is that of estimating the magnitude of the wind forces to which structures will be subjected. So far as I can ascertain, this problem first arose in an acute form in connection with the destruction of the Tay Bridge in a severe storm in 1879. An investigation was made under the aegis of the Meteorological Office and the Meteorological Society, the work being done by Mr. W. H. Dines. It was this investigation which led to the design of the Dines pressure-tube anemometer, and which demonstrated the fundamental importance of placing anemometers in strictly comparable exposures. In regard to the effects of wind on bridges, the changes of direction of the wind are perhaps more important than the changes of speed of the wind, and the meteorologist is now in a position to supply such information from a large number of stations in the British Isles.

(b) *Heating of buildings*

The heating of buildings raises a problem which is purely meteorological. Simply stated it is this :— what will be the worst conditions in which the heating installation must be capable of maintaining comfortable conditions within the building ? It is a frequent practice among engineers to assume that the lowest daily mean temperature to be taken as a basis for the design of space-heating apparatus is from 30° to 32°F. This figure is admittedly unrealistic and is too high for many purposes, and now requires reconsideration. Here is a problem in the consideration of which the meteorologist must play a leading role. It is to be studied, in the near future, for the British Isles by a Committee of the four Engineering Institutions most closely concerned.

A cursory examination of the mean daily temperatures for the period 21 January to 8 March 1947, suffices to throw serious doubt on the above mentioned basis of outdoor temperatures 30° to 32°F. In London, the mean daily temperature over this period was 30·4°F. Of the 47 days in that period, 23 days had mean temperatures below 30°, 11 days below 28°, 3 days below 26°, and 1 day below 21°F. At Manchester the mean temperature for the whole of the same period was 29·4°F, and of the 47 days there were 23 days below 30°, 12 below 28°, 5 below 26°, 3 below 24°, and 2 below 22°F. Thus while the incidence of cold weather increases northward, the difference is at first sight not very great, though it is in fact significant.

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Weather and food

By Sir WILLIAM GAMMIE OGG
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The rapid increase of population in many parts of the world has again brought into prominence the problems of food production. At last year's meetings of the British Association and at the recent United Nations Conference on the Conservation and Utilization of Resources held at Lake Success, much attention was given to improvements in crops and agricultural techniques and to the possibility of bringing the undeveloped areas of the earth's surface into agricultural production. At these meetings insufficient stress was laid on the importance of climate (including weather) but Sir Robert Watson-Watt in his presidential address to the Royal Meteorological Society this year emphasized the need for wider study of biological meteorology. Climate plays a decisive part in soil formation and utilization and affects the yield and quality of crops, the intensity and spread of plant diseases and insect attacks and the health and performance of animals. A more appropriate title for this paper might have been Climate and Agriculture. I propose to deal particularly with questions relating to soil.

There are very many different kinds of soil found throughout the world and the type which develops in any particular region depends on five main factors, parent material, climate, vegetation, topography, and time. The character of the soil is determined by the operation of processes dependent on climate as modified by topography. These processes act both directly and through vegetation and other biotic factors. The soil parent material itself is the result of weathering processes on a parent rock and this weathering may have occurred under climatic conditions quite different from those of the present day. In general, climate and vegetation influence the characteristics of soil over broad regions whilst the other factors tend to give rise to more local differences. This is borne out by the distribution of the major soil groups of the world, most of which occur in belts corresponding roughly with the zones of climate and vegetation. In addition to the zonal soils, there are intrazonal soils in which the effect of some local factor of parent material or topography predominates and azonal soils in which, through lack of time and other causes, normal development of the soil profile has not occurred.

The chief members of the zonal group are the podzolic soils, the chernozems, the lateritic soils with associated red and yellow earths and the desert soils. The soils of Britain belong mainly to the podzolic group. Under natural conditions they develop in a cool moist climate under coniferous forest or heath. The topmost layer is often peaty in character and beneath this is a layer of decomposing humus. Next comes a layer rich in silica and then another containing an accumulation of iron and alumina. The brown forest soils in Britain form an intermediate group between the more leached podzolized soils and the less leached soils of dried climates.

A very different group is the *chernozems* or black earths which develop under a steppe vegetation. The top layer is rich in humus and there is a gradual lightening in colour downwards and finally a layer rich in calcium carbonate. There is sufficient leaching to remove the most soluble salts but not enough to wash out the calcium and magnesium carbonates and all the zonal soils of the semi-arid and arid regions have a layer of carbonate accumulation. The chernozems are rich in plant nutrients and have excellent physical structure but they occur in regions of rather low rainfall and this somewhat limits their fertility. In some regions unsuitable agricultural practices have led to the loss of organic matter in these soils and the consequent loss of soil structure has brought about serious erosion problems. Erosion is also prevalent in other soil types in many parts of the world and there is urgent need to study the properties of the various soil types and to adopt agricultural practices to suit them.

The lateritic soils, red earths and yellow earths develop under humid tropical climates where weather and biological processes are intense. Through absence of glacial interference during recent geological time, these soils have been subject to uninterrupted weathering for a very long period. They have been insufficiently studied and attempts to utilize them for agriculture have led to unexpected and difficult problems. In the African groundnut scheme, for instance, the soil in one region sets very hard after the natural vegetation has been cleared.

The desert soils are a rather ill-defined group developed in arid regions under a scanty vegetation. The weathering is mainly physical and the soils are generally light coloured and unleached. Erosion is sometimes intense, particularly wind erosion, and in some areas, the finer material is swept away leaving a layer of stones and pebbles, often referred to as the " desert pavement." There are also large areas of shifting sand, partially stabilized sand dunes and, in the sub-surface, crusts of calcium carbonate, gypsum or ferruginous material sometimes occur.

Saline soils, an intra-zonal group, are associated with areas of impeded drainage in semi-desert regions but these soils can be used for agriculture if they are irrigated and drained. Those which contain soluble neutral salts (*e.g.* $NaCl$ or $CaSO_4$) are usually structureless. Where there is a large amount of sodium present subsequent leaching may lead to the formation of sodium carbonate and high alkalinity. At this stage a pronounced structure develops in the soil and the subsoil tends to become very compact and impervious.

These are some of the main world groups of soils and it is evident that climatic factors, particularly rainfall in relation to evaporation, play a very important part in their development. Rainfall affects not only the supply of moisture but also the aeration and the extent of leaching. Temperature affects evaporation and the velocity of chemical weathering and has a great influence on microbiological activity. In cold and in dry regions physical weathering is far more important than either chemical or biological weathering, whereas in moist, hot regions the reverse is the case. The indirect effect of climate through vegetation is also of great importance. In studying the processes of soil formation, in evolving a system of classification and in carrying out soil surveys, further climatic data would be of much value.

In the practical business of farming, climate is equally important. The natural soils can, to varying degrees, be modified by treatment to meet the farmer's needs. Those which have been leached can be improved by the application of lime and manures, swamps can be drained, arid regions irrigated and saline soils brought into use by a combination of drainage and irrigation. Climatic data are, again, of great value in connection with these operations. The amount of leaching, for instance, affects the quantities of lime and manures required and in land drainage it is important to know the amount of water which must be carried away at different times of the year. The draining of agricultural land is still an art rather than a science and follows rule-of-thumb methods but with fuller knowledge of the soil properties and the climate it should be possible to make much more accurate estimates of the depth and distance apart at which the drains should be laid. Regional drainage schemes for catchment areas involving such measures as the building of dams to cope with surface run-off and underground drainage also involve the use of meteorological data. Disastrous floods have occurred in many parts of the world where forests have been cut down and bare soil has been exposed to heavy rainfall. These difficulties could have been foreseen had the climatic conditions been known and taken into account, and some attempts are now being made to correct past mistakes. An example of this is the vast project of the Tennessee Valley Authority which has already secured a large measure of flood control and enabled much better use to be made of large areas of valuable agricultural land. At the same time the electrical power produced at the dams will possibly pay for the whole scheme.

The practices which have led to flooding have also been a major cause of soil erosion. Under natural conditions the regions of high rainfall have the greatest amount of vegetative protection but the balance between soil erosion and soil formation is upset by the clearing of land for agriculture and by the introduction of agricultural practices unsuitable for the type of soil and the climatic conditions. Deforestation and the continuous growing of crops which expose bare soil to heavy rainfall or wind are potent factors, and erosion is further accelerated by the reduction of the soil organic matter and the destruction of the soil structure. Soil erosion is a serious menace in many parts of the world and much of it could have been avoided if attention had been paid to soil and climatic conditions. The Dust Bowl in the United States is an example of what can happen and what has already happened to vast areas of fertile surface soil and steps are now being taken to avoid similar mistakes in the future.

Water supply is probably the most significant single factor in agricultural production and over much of the earth's surface, rainfall is insufficient for satisfactory crop growth. From very early times irrigation has been practised and as the world's population increases, intensified efforts will have to be made to utilize the arid and semi-arid land. The first essential is to provide adequate supplies of suitable water and in this, again meteorological data are necessary. The provision of irrigation water for one area may unduly restrict the supplies to another and the schemes must therefore be considered on a broad regional basis. One of the most serious problems in irrigation is that of alkali. Soils in arid regions often contain harmful quantities of salts, the rainfall having been inadequate to leach them out. Application of irrigation water tends to concentrate the more soluble salts where ground water comes near to the surface and this has led to the abandonment of many irrigation schemes in the past. The remedy is to secure a proper balance between rainfall and irrigation on the one hand and evaporation and drainage on the other. Irrigation water is often scarce, or at any rate expensive, and to use too much may be wasteful and even disastrous. In this connection it should be noted that evaporation from crops is almost entirely a weather-controlled phenomenon and the physics of the process is being studied by H. L. Penman at Rothamsted and by various others. Soil moisture deficits have been calculated and water applied on this basis in experiments on the irrigation of sugar beet carried out at two centres in South-East England during the past three years. It was found that the calculated amounts gave satisfactory results and that yields were not increased and, in fact, in some cases, were decreased by using double the amount of water.

From calculations of soil moisture deficits it is probable that the crops over a considerable area of Southern England — south-east of a line from the Severn to the Humber — could use 4 to 6 in. more water than falls as rain (unpublished work from Rothamsted Experimental Station). In a dry year, such as 1949, even a deep-rooted crop like sugar beet benefits from irrigation and shallow-rooted crops will benefit in nearly all years.

The world pattern of climate dictates broadly the pattern of farming. We have seen how it influences the types of soil which occur and how it concerns drainage, erosion and irrigation. It also largely determines the types of crops which can be grown and affects their yield and quality. The three most important factors in climate from the point of view of crop-growth are temperature, water supply and light. They are closely inter-related but temperature broadly determines the belts or zones in which the various crops can be grown and water supply their distribution within these zones.

The climatic range of crops has been greatly extended through the efforts of the plant breeder and in Canada, for instance, the growing of wheat, maize and soya beans is being steadily pushed north. Frost damage is a major hazard in many regions, especially for the fruit grower, and requires further study.

Climate also affects the quality and composition of crops, for instance the sugar content of sugar beet and the nitrogen content of barley. The subject was studied at Rothamsted for many years by Lawes and Gilbert and in papers published in 1880 and 1884 they showed that the yield and composition of wheat grown on the same plots with constant manurial treatments varied widely according to the season. A great deal of work has been carried out since that date but much still remains to be done to distinguish the effects of specific weather factors on composition.

Climate and weather influence the incidence and spread of plant diseases. The conditions may make the host plant more receptive or they may suit the disease organism itself or some insect vector. Potato blight and rusts in cereals, for instance, are favoured by moist warm weather which facilitates the production, dissemination and germination of spores. In the case of virus diseases weather conditions influence both the susceptibility of the plants and the numbers of insect vectors. Work on this subject and on weather in relation to insect pests is in progress at Rothamsted. Systematic trapping of insects over a period of years has demonstrated that, in general, they are more abundant after wet weather in summer and warm weather in winter. From this, a method of forecasting insect abundance has been developed. The drift of insects in the upper air is also being studied.

Climatic conditions are important in livestock production and have also a bearing on certain animal diseases. Temperature is perhaps the most important factor and difficulties have been experienced in introducing breeds of animals from temperate to tropical and sub-tropical regions. Some progress has been made by cross-breeding. For instance, Brahman (Zebu) types of cattle which withstand tropical conditions have been crossed with more productive breeds from temperate climates.

Weather conditions affect not only the policies and programmes of farmers and horticulturists but also their day-to-day work, particularly at sowing time and harvest when losses due to adverse conditions may be very great. In this connection weather forecasts are of great value and longer-term and better forecasts would add materially to our food supplies. Weather studies in relation to food production are still in their infancy and farmers and agricultural scientists alike welcome the increasing interest shown by meteorologists in their problems and the establishment by the Meteorological Office of a branch dealing with agricultural meteorology.

Discussion

Mr. N. R. Hagen (*Meteorological Attaché, U.S.A.*) : I have been asked by Dr. Reichelderfer to express his deep regret at being unable to come here to participate in these interesting symposia. Unfortunately, he was obliged at the last moment to remain in Washington on urgent administrative matters, and has telegraphed me to extend to all his colleagues of the Royal Meteorological Society his warmest congratulations and best wishes.

Your Chairman asked me this morning to comment briefly on Meteorology and the Community as practiced in my country.

The United States now has a population of 150 million people and the annual Weather Bureau appropriations total about $25,000,000, which means that our service costs each citizen about 17 cents a year. For this relatively small cost the Weather Bureau provides each citizen the usual, but important, warning service against floods, hurricanes, storms, frost, sleet, temperature extremes, *etc.*, as well, of course, as an extensive forecast and weather reporting service.

Each day our weather forecasts are printed in about 1,500 daily newspapers, and are broadcast several times a day from more than 800 radio stations, many of which are made directly from microphones located in Weather Bureau field stations. In addition, 24-hour service to the public is available at approximately 400 stations.

An especially useful and increasingly popular service is our automatic telephone weather service now installed in 7 of our largest cities. As you may know, this automatic service permits several dozen subscribers to listen simultaneously to a forecast transmitted by a magnetic tape recorder. The total number of telephone calls for forecasts in all 7 cities averages about 300,000 a day. On more than one occasion of extreme weather, telephone weather forecasts have been given to over 250,000 callers in a single city in 24 hours. It will be understood, of course, that before the automatic telephone was installed in these cities, the average number of individual requests for forecasts that could be handled on a personal basis was of the order of one thousand a day. This is a sample indication, I feel, of the extensive interest a community has in weather, and also suggests the vast potential most weather organizations have in extending their services to all the community.

Some services provided in the United States that perhaps are not in general use in most countries are general weather forecasts and charts transmitted by television stations, landline and wireless facsimile transmissions of weather charts, routine issue of five-day weather forecasts twice a week, and the issue, on an experimental basis, of 30-day weather outlooks.

In the United States, a further interesting development has occurred during recent years in the field of private and industrial meteorology. There are many private and quasi-public concerns in the United States which have an extensive need for detailed and continuing meteorological services. When the general services provided by the Weather Bureau do not cover adequately their special requirements, they are encouraged to secure the services of a private meteorologist, who often, because of his fuller understanding of how meteorology affects his concern's prosperity, can suggest action resulting in greatly increased operational efficiency and economy.

In order to measure the value of our services, as well as to justify our appropriations to Congress and the tax-payer, the Weather Bureau often makes surveys of the amount of savings that have resulted to shippers, agriculturists, and others as a result of timely and detailed warnings or forecasts. Many of the services provided cannot be evaluated in terms of money savings, of course, but there is little doubt that hundreds of lives and hundreds of millions of dollars of property are saved each year in the United States because of the service provided by the Weather Bureau.

In the last few years I have visited more than 40 National Meteorological Services and naturally have noted the many and diverse ways they have been serving their states and their citizens. As a result of such visits I strongly believe that most of them, even during these days when economy of government expenditures is a prime necessity, have the fullest justification in soliciting increased financial support for an expanded service.

Communities gradually, but far too slowly, are becoming aware of the valuable public services that can be performed by a modern and well-organized meteorological organization. Methods whereby this community-educational process could be speeded up merit further attention and call for the combined efforts of Meteoroological Directors, meteorological societies and meteorologists generally.

PROFESSOR O. G. SUTTON (*Military College of Science*): As Chairman of the Atmospheric Pollution Research Committee I desire to endorse Sir David Brunt's words about the importance of this work. When Shaw and Owens wrote their book many people were inclined to regard attempts to reduce atmospheric pollution as little more than laudable efforts to reduce a nuisance. With growing industrialization and particularly with the advent of atomic energy, atmospheric pollution has now become a vital matter. There have been serious accidents in recent years in Belgium and U.S.A., and I think that the fact that we have escaped such disasters in this country is more a matter of good fortune than of design. The solvable problems of meteorology are not numerous, but many of those arising in atmospheric pollution could be brought within the scope of mathematical physics with important consequences for those designing industrial plants. Regarding pollution from chimneys, we now know with fair precision where the 'hump' of concentration occurs when the atmosphere is in neutral equilibrium, and we have a rough idea how the 'hump' moves downwind in inversions, but much remains to be done. I appeal to meteorologists to take an interest in this work, so essential for the health of the community. Finally, I should like to call attention to certain problems of micrometeorology, which enter into the work of biologists. The atmosphere within a centimetre or so of the ground is in many ways an unknown region. The biologist and the plant breeder need information about the climatology of Lilliput, as well as that of the world of human beings.

PROFESSOR SIR DAVID BRUNT (*in reply*) : Mr. Hagen left me with the impression that Washington came before the Weather Bureau. Had the Weather Bureau come first Washington might have been placed in some more suitable climate.

With reference to Sir William Ogg's remarks on the improvement of soils in the arid and semi-arid zones of the world, I learned at a conference held by U.N.E.S.C.O. in Paris in December 1949 that in Israel, Professor Picard, who is in charge of the arid-zone work, has found plentiful supplies of underground water in the heart of some of the most arid regions of his country. The discovery of underground water supplies would appear to be the key to the whole problem of arid zone improvement.

The President asked me to amplify what I said concerning Gideon's fleece. Gideon made use of the fact that dew can come out of the air, or out of the ground. In the first case dew would form first on the fleece, while in the second, the fleece would remain dry, since the skin would remain on the fleece. Moreover, an able man, who must have slept on a fleece many hundreds of times, must have learned from observation the necessary facts.

DR. HELGE PETERSEN (*Denmark*) : I should like, with reference to the paper read by Sir William Ogg, to draw attention to the fact that drainage may be too effective.

In Denmark there has been very strong propaganda in favour of drainage, which has resulted in a very intensive draining. That has led to the result that the lower parts of the fields, which previously in a wet spring became small ponds unfitted to receive the seed, can now produce a fine harvest. But the intensive draining has also the effect of sinking the ground water level, so that in dry summers, the plants cannot get sufficient water supply from the ground.

Actually it has been our experience in Denmark, during the dry summers of some years ago, that the water supply has failed in many places, so that the wells became empty, because of the lowering of the ground water level.

DR. H. L. PENMAN (*Rothamsted Experimental Station*) : As Sir William Ogg has told us, agricultural meteorology is a study still in its infancy, in which workers are few and problems are many. Basically, these problems are more complex forms of those met in ordinary meteorological physics, and any physicist who seeks an outlet for his ability can be assured that he will find in agricultural meteorology a worthwhile challenge on the fundamentals of his own subject.

To some who may accept the challenge a warning will be necessary. Past experience indicates that it is largely a waste of time to collect weather records without having a definite object in view. Questions should come before answers — and preferably should grow out of biological problems — and then decisions can be made about what to measure, the accuracy required, and the technique to use.

Mr. A. H. Yates (*College of Aeronautics*) : Contributions to the knowledge of vertical currents in the atmosphere are being made by glider pilots. The vertical currents beneath and inside cumulus clouds are used and, recently, standing waves in the atmosphere have been used to enable sailplanes to reach 14,000 ft above M.S.L. in Great Britain and more than twice as high in U.S.A.

It is of vital importance to glider pilots to have prompt reports of upper air temperature and velocity gradients and the service of ' AIRMET ' has been invaluable in this connection. The recent loss of their wavelength was a severe blow to the gliding fraternity and I hope that the Society will do all in its power to see that the AIRMET transmissions are resumed.

Dr. G. L. Hogben (*I.C.I.*) : I should guess that not more than two per cent of listeners in the community understand the weather messages aimed at them. There is a risk of meteorologists assuming that the whole community understand a set of expressions which are in fact completely understood by meteorologists only. Weather organisations have a great need of modern public-relation techniques in order to help their relations with the community. For their own continued existence and expansion depend on the existence of an expanding body of opinion which is educated to know the value of, and to understand the weather service.

Mr. R. Y. Sanders (*B.E.A.*) : Measurements of the effect of weather conditions on the community are important if research is to be usefully directed. For the value of meteorology to be properly appreciated by the community — and especially by the financiers and politicians — the most effective measurement is in terms of money values, and as regards the vegetative effects of weather conditions, it should not be beyond statistical resources for instance to calculate, on standard acreages and money values, the effect on national or world prosperity of changes in crop yield per acre.

In respect of what may perhaps be termed the thermo-dynamic effects of weather, perhaps the most comprehensive form of measurement available in this country is the load record of the National Grid System. In every half-hour of every day, in the seven Grid Control Centres already referred to by Sir David Brunt, there is recorded the aggregate load on all the generating stations serving that Control Area. Nowadays some 80 per cent of the electricity requirements of industry are furnished by public electricity supply, and about 86 per cent of all households and 90 per cent of all commercial premises are connected to the system. Over half the households have electric space-heaters of one form or another, about one-fifth cook electrically and over one-tenth use electricity for water heating — a surprisingly large figure, as well over 40 per cent of all households have no piped hot water supply. It is not surprising, therefore, that the load on the Grid system is affected by weather conditions to an ever-increasing extent and is a remarkably sensitive indicator of the effects of such conditions on the life of the community.

There is a very strong seasonal variation in Grid load, the normal summer weekday demand being of the order of 7 million kilowatts, whereas the normal winter demand is about 11 million kilowatts. This seasonal variation is not at all a bad thing, since plant has to be overhauled and a higher relative plant capacity would be required if the summer dip in load did not accommodate the necessary plant withdrawals. Short-term fluctuations are in a different category ; here two groups of weather factors have to be distinguished. First, those which give rise in the hours of daylight to increased use of artificial lighting ; a heavy thundercloud crossing London in the middle of a summer day may cause a rise of load within half an hour of well over a quarter of a million kilowatts — and plant has to be maintained in service to meet such contingencies. The other group is the complex of weather factors affecting the human sensation of warmth comfort ; severe cold weather may well add over a million kilowatts, or about 10 per cent, to the national load.

CDR. C. E. N. FRANKCOM (*Meteorological Office, London*) : Sir Nelson Johnson referred to weather information for the shipping and fishing industries. When one considers the high cost of building and repairing ships, at present, the necessity for doing all that is possible for the safety of shipping is obvious. Despite improvements in propulsion and design of ships and all the modern scientific aids to navigation, a surprisingly large number of shipping casualties are still due, either directly or indirectly, to the weather. It is very appropriate, therefore, that Meteorological Services throughout the world should do their best to improve their service of weather information for shipping, and there is no doubt that shipmasters and shipowners are becoming increasingly aware and appreciative of the value of this service.

Another aspect of shipping in which the meteorologist could give practical help is in giving advice about the ventilation of holds and safeguarding of cargoes from damage, due to humidity and other factors. When, as at present, there is a world food shortage, it is particularly important that everything possible should be done to see that cargoes are carried in good condition, but there is no doubt that enormous damage is done each year in ships' holds by sweat and other similar causes. The problem is not an easy one, for it not only involves questions of the humidity and temperature of the air itself, both outside and inside the hold, but sea temperatures and the nature of the cargo itself, for many cargoes have somewhat complicated hygroscopic qualities. It seems that perhaps this is a question which can be partly solved by some form of meteorological education and by liaison between meteorologists and the shipping industry.

With regard to Sir David Brunt's paper, there are many problems akin to engineering in the marine field in which the meteorologist can be of considerable help. In the realm of naval architecture, for example, there is the problem of ships which owing to some weakness may develop a fracture amidships, due to being poised on the crest of a large wave at their mid-length, or two waves at their extremities. Cases have arisen in which ships have foundered without survivors owing to what can only be surmised to be this cause. Although waves are not strictly meteorological, their study comes readily into the field of that science, and the meteorologist might well give very useful information about the generation and structure of waves to ship designers.

The meteorologist could also give useful information about precautions to be taken in the location and protection of harbours. In this connection, such matters as sea and swell and seasonal winds might be mentioned.

Another rather unusual application of meteorology is in connection with the stability of ships. A ship with a fairly high centre of gravity (*e.g.*, one loaded with a deck cargo) might be quite safe in still water but in a sea-way with a strong wind on the beam she might easily develop a list, and some cargo might shift a little as a result. As long as that wind remained more or less steady in force, the ship might not come to any very great harm, but a very gusty wind might, under certain circumstances, have disastrous results upon the stability of that vessel, and cause her even to capsize. Information about the structure of wind over the oceans would perhaps be of value to ship designers.

It is appropriate that shipping should find a place in this symposium, not only because the British are an island race and their whole history and their present and their future is inevitably connected with ships and seamen, but also because organized meteorology in this country arose out of the needs of shipping nearly a century ago. That great American seaman, Matthew Fontaine Maury, was the convener of the first International Meteorological Conference at Brussels in 1854, and as a direct result of this Conference the British Meteorological Office first saw the light of day in 1854 under Admiral FitzRoy.

CENTENARY SYMPOSIUM
ON
ATMOSPHERIC TURBULENCE

Held at London on the morning of 3 April 1950

Chairman : Professor Sir DAVID BRUNT, F.R.S.

551.511, 551.524.1, 551.551

Discussion

Professor P. A. Sheppard opened the discussion by giving a preliminary account of research in this subject which was being done in the Department of Meteorology at Imperial College of Science and Technology.

The word turbulence, he said, can be regarded as defining a technique for the determination of the transport of physical entities, such as heat or matter, by motions which, though not necessarily small themselves, are yet smaller in scale than those under immediate consideration. This is an obvious extension of Reynolds's original approach to the problem and is in contrast to the more common view that the study of turbulence implies the investigation of fluid flow patterns which are so small and complex that only their statistical structure is capable of experimental determination and profitable interpretation. In meteorology some flow patterns are large compared with the instruments used so that they can be investigated individually—they are on the so-called synoptic scale ; nevertheless their structure (and the transport to which they give rise) can still be usefully expressed in statistical terms. The approaches through 'transport' and 'structure' are formally equivalent in that they can be determined by the same observational technique.

For a conservative entity s per unit mass of fluid we assume that we can separate its variation, in time and space, into a mean component $\bar{s}$ and a more rapidly varying component or fluctuation s' such that

$$s = \bar{s} + s' \qquad (1)$$

where $\bar{s}$ and s' satisfy various ' meaning-rules ' such as $\bar{\bar{s}} = \bar{s}$, $\overline{s'} = o$. The mean values, which may be taken over time, space, or mixed time-space intervals, are here assumed to be taken over time.

It is instructive to consider some special cases of the expression :

$$R_{AB}(\mathbf{l}, \tau) = \overline{s'_A(\mathbf{r}, t)\, s'_B(\mathbf{r}+\mathbf{l}, t+\tau)} \qquad (2a)$$

or the normalised equivalent

$$\rho_{AB}(\mathbf{l}, \tau) = \frac{\overline{s'_A(\mathbf{r}, t)\, s'_B(\mathbf{r}+\mathbf{l}, t+\tau)}}{\overline{\{[s'_A(\mathbf{r}, t)]^2 [s'_B(\mathbf{r}+\mathbf{l}, t+\tau)]^2\}}^{1/2}} \qquad (2b)$$

Here s'_A is the fluctuation in some entity s_A at a point in space defined by $\mathbf{r}$ and in time defined by t, $s_B{}'$ is the fluctuation in some entity s_B at a point in space defined by $(\mathbf{r}+\mathbf{l})$ and in time defined by $(t+\tau)$ and the overbar implies a mean value involving an integral, with respect to time, of the products concerned. The functions R and ρ are assumed to be much more strongly dependent on $\mathbf{l}$ and τ than on $\mathbf{r}$ and t.

If $\mathbf{l}$ and τ are both zero, and if s_A represents a component of the fluid velocity in a particular direction, then R_{AB} is proportional to the *turbulent flux*, *i.e.* the flux due to the fluctuations, of s_B in the direction of s_A.

When s_A and s_B represent the same entity three cases of interest arise. If both $\mathbf{l}$ and τ are zero R_{AA} is the *intensity* of the turbulence. If τ be zero $\rho_{AA}(\mathbf{l})$ is a *spatial correlation*, while if $\mathbf{l}$ be zero $\rho_{AA}(\tau)$ is an *auto-correlation*, which is of special interest since it is uniquely related to the *energy-spectrum* of the turbulence. Precisely, if $F(n)\,dn$ be the fraction of energy (used in the sense of intensity, above) between frequencies n and $n+dn$

$$\rho_{AA}(\tau) = \int_0^\infty F(n)\cos 2\pi n\tau\,dn \qquad . \quad . \quad . \quad . \quad (3a)$$

$$F(n) = 4\int_0^\infty \rho_{AA}(\tau)\cos 2\pi n\tau\,d\tau \qquad . \quad . \quad . \quad . \quad (3b)$$

Further details of the statistical structure of the turbulence can be obtained from this and more complicated correlation functions, but enough has been said to link the two approaches. From a practical point of view it is clear that a vast amount of information, both as to transport and structure, can be obtained by determining integrals of products of turbulent fluctuations.

Professor Sheppard went on to talk of instruments which have been developed for recording atmospheric turbulent fluctuations and of the mechanical computer with which it is hoped to evaluate integrals of their products. A fuller description of these and of results already obtained will be published later. It is intended that observations shall be made over land and sea ; a start has been made by recording fluctuations of temperature and humidity in the lowest ten metres of the atmosphere over Lough Neagh, Northern Ireland, in April 1949. He concluded by showing slides of some of the results obtained by analysing these fluctuations.

Further results were shown by H. Charnock, who compared them with work published by American and Japanese investigators. Significant differences are due, he said, not to differences in the turbulence so much as to differences in the way in which the mean value was taken. The definition of the mean value is a problem which, although it must be well known to workers in this subject, has received surprisingly little discussion in the literature.

The aim of this investigation into atmospheric turbulence is first to filter the observed variations of one or more entities into high and low frequency components, then to express statistical properties of the faster components in terms of the slower. In applying the filter, the precise definition of the meaning-process presents a theoretical and a practical problem. In the records shown — indeed in most published work — the mean value has been assumed constant over the particular length of record analysed. This is convenient in practice but implies that the mean value is discontinuous, whereas it is clear that to have any definite significance in, say, an energy equation the mean value must be continuous over time and space.

Reynolds's original method of defining the mean value

$$\bar{s} = \frac{1}{\Delta t}\int_{t-\Delta t/2}^{t+\Delta t/2} s\,dt \qquad . \quad . \quad . \quad . \quad . \qquad (4)$$

ensures that $\bar{s}$ is continuous, while the arbitrary nature of Δt allows some choice of the scale of turbulence to be investigated. Values of the intensity of the temperature fluctuations $\overline{T'^2}$ have been computed from the Lough Neagh records using this 'running-mean' technique and are found, as one would expect, to increase steadily as Δt increases. The slope of the graph of $\overline{T'^2}$ against $1/\Delta t$ also increases steadily as Δt increases, indicating that a large fraction of the 'energy' is associated with the low frequency components of the motion. The graph cannot be treated as a precise energy spectrum since an unweighted running-mean used as a filter has a complicated frequency response.

The question naturally arises as to whether there is any special merit in adopting Reynolds's technique of meaning rather than in using as a mean value the sum of the Fourier components with frequency less than some arbitrarily chosen value. The latter method has various practical advantages as well as the theoretical merit that it avoids to a large extent logical difficulties associated with the 'meaning-rules' mentioned by Professor Sheppard — Reynolds's method of meaning and the meaning-rules quoted above are obviously incompatible in the general case.

Finally, Mr. Charnock showed the results of a determination of the 'energy' spectrum of the temperature fluctuations recorded over Lough Neagh. The spectrum, which has been obtained by using the wave analyser at the Admiralty Research Laboratory, was effectively continuous from wave period 0·5 to 36 seconds, the limits being imposed by the analytical technique and not by the turbulence. Observations are now being analysed to extend the spectrum from fractions of a second to tens of years.

H.C.

Professor O. G. Sutton (*Military College of Science*): I very much welcome the work done by Asst. Professor Sheppard and his team, because it supplies a real need. In many branches of meteorology observational data were, if anything, too plentiful, but in studies of turbulence in the lower atmosphere there seem to be more theories than facts. I have frequently found it very difficult to get hold of reliable observations with which to test theoretical deductions — readings from ordinary meteorological instruments are usually of little value in resolving the delicate questions which arise in this sort of work. Regarding mean values it is worth emphasizing that the rules adopted by Reynolds for the formation of means are by no means axiomatic. If we take

$$\bar{u} = \frac{1}{T}\int_{t_0-\frac{1}{2}T}^{t_0+\frac{1}{2}T} u\,dt\,; \quad u' = u - \bar{u},$$

then, in general

$$\bar{u} = f(x, y, z, t_0, T)$$

where T is the time of sampling. If the mean velocity constructed in this way is to be representative of the motion as a whole, the period of sampling T must be long enough to ensure the inclusion of an adequate number of oscillations, but the universal adoption of a very long period of sampling might mask important changes which are taking place in the motion, *e.g.* if the general level of velocity is steadily rising or falling during the period chosen, the motion could not be properly represented by a single constant mean velocity. In wind-tunnel work it is true that the mean value is independent of the period of sampling, once this exceeds a certain value, but one of the great difficulties of atmospheric turbulence is that there is no effective upper limit to the size of eddies, so that as the period of sampling is increased, disturbances which do not appear to affect the short period means become effective. If conditions are such that the mean velocity is steady, the analysis is greatly simplified and it follows that $\overline{\overline{u}} = \overline{u}$ and so $\overline{u'} = o$. If the mean flow cannot be regarded as steady, some restrictive condition must be imposed, *e.g.* that changes in u are sufficiently rapid and of such a nature that a time T can be defined for which $\overline{u}$ varies only slowly. In all cases, the equation $\overline{u'} = o$ must be satisfied, either exactly or to a high degree of approximation, before the Reynolds analysis can be applied, and this is equivalent to assuming that some measure of regularity exists, even in turbulent motion.

I am very interested in the curve showing temperature fluctuations against height in the lapse period. In 1947 I proposed a theory of natural convection (*Quart. J. R. Met. Soc.*, **74**, 1948) which predicted that these fluctuations would decrease with height according to the law $\surd(\overline{T'^2}) \propto z^{-0\cdot4}$ (z = height). From a brief examination of the graph shown it is clear that this prediction was fulfilled more or less exactly. In the original paper I have found evidence to support this law from Johnson and Heywood's observations at Leafield, but many of those who took part in the discussion objected to this, pointing out (quite correctly) that the instruments used by Johnson and Heywood were not designed to record fluctuations and also that there was some doubt concerning the effect of instrument lag. It was pleasant to see both the theory and the observations vindicated by the present results.

Mr. J. R. D. Francis (*Imperial College*) : One object of the Lough Neagh experiments referred to earlier was to find how the shear stress of the wind on a water surface varies with windspeed, fetch, and size of waves. Although the exposure and instrumentation were good, the wind velocity profiles did not appear as straight lines when velocity was plotted against log (height). There were often kinks between 1m and 3m above water level. The problem therefore exists how to find which part of the velocity profile determines the shear stress.

Some experiments have been done with a wind tunnel 20 ft long and 3 in. wide with 6 in. water below a 9 in. air space. Profiles taken with a pitot tube show serious kinks which under some conditions can give a layer of slow air between layers of fast air. The kinks thus appear to be real, and due neither to instrumental nor exposure errors. Some modification of the usual " rough surface " boundary layer theory is therefore required to find what shear stress exists at the water surface. It is possible that the slope of the portion of the velocity profile several metres above the water surface is that which determines the shear stress.

Professor N. N. Faegri (*Bergen Museum*) : In connection with the discussion of problems of atmospheric turbulence I should like to draw attention to the results of an investigation which was carried out for a quite different purpose, *viz*. the problem of long-distance pollen transport. By very simple apparatus the pollen grains falling at Weather Ship station M (66° N. 2° E.) were caught in fortnightly periods. The numbers encountered were astonishingly high, *e.g.* oak *ca* 1 grain per cm^2 per season, pine and birch *ca* 5. As we can trace the origin of the pollen grains, and as their rate of fall is known, the results can be utilised to give a very rough indication of the large-scale vertical atmospheric turbulence.

The Department of Meteorology at Imperial College was open for inspection following the discussion.

Centenary meeting

3 April 1950

On the afternoon of 3 April 1950, nearly 200 members and overseas delegates assembled in the Society's rooms for the centenary meeting.

At 3 p.m., Sir Robert Watson-Watt, C.B., F.R.S., reminded his audience of the inauguration of the Society at Hartwell House, near Aylesbury, just 100 years ago. The President then read a loyal message sent to the Patron, H.M. The King :—

To The King's Most Excellent Majesty :

May it please your Majesty : the President, Council and Members of the Royal Meteorological Society, now assembled with their guests from overseas in Christ Church, Oxford, in a symposium on scientific meteorology, present their humble duty to your Majesty from this first part of the celebration of the hundredth anniversary of the formation of the Society. The British Meteorological Society was founded on the third day of April 1850, and was honoured by Her Most Gracious Majesty, Queen Victoria, by the granting to the Society of a Royal Charter of Incorporation in 1866 and by Her permission to use the style of " The Royal Meteorological Society " from 1883.

The Society tender to your Majesty the Patron, their most loyal and most grateful thanks for your Majesty's gracious patronage and for the assurance of interest in the Centenary Celebrations which your Majesty was pleased to cause to be conveyed to them. They beg leave to pledge themselves to the continued progress of a science which already owes much to distinguished public servants and individual subjects of your Majesty and the fuller applications of which are of profound importance to the nations and communities which are linked by their respectful and affectionate loyalty to your Majesty's person.

I remain, with profound veneration,

Your Majesty's most faithful subject and devoted servant,

28 March 1950. Signed : ROBERT WATSON-WATT

and His Majesty's most gracious reply :—

I shall be grateful if you will express to the Council and members of your Society my sincere thanks for the kind and loyal message which you have addressed to me. On its foundation, 100 years ago, my great grandfather, the Prince Consort, gave practical proof of his interest in the science of meteorology and of his belief in its value to mankind. Now, after the lapse of a century, I can testify, from my own experience as a sailor, as an airman, and as a farmer, to the great contribution which meteorologists, both in peace and war, have made to the common good.

I warmly congratulate the Society on the record of achievement established on this anniversary, and as its patron wish it all success in the future.

29 March 1950. Signed : GEORGE R.

Congratulatory Messages from the following were then read :—

GREAT BRITAIN : Geologists' Association ; Institute of Physics ; Royal Astronomical Society ; Royal Botanical Gardens, Kew ; Royal Entomological Society ; Royal Photographic Society ; Messrs. Negretti & Zambra, Ltd. ; and several of the Society's own Fellows who were unable to be present.

COMMONWEALTH :

Canada : Minister of Transport, Ottawa.
East Africa : Meteorological Department.
New Zealand : Air Department, Meteorological Branch.
South Africa : Weather Bureau.

FOREIGN :

Austria : Central Institute for Meteorology and Geophysics ; Academy of Sciences. *Azores Islands* : National Meteorological Service.
Belgium : Royal Meteorological Institute.
Brazil : J. de Sampaio Ferraz.
Czechoslovakia : State Meteorological Institute ; Institute of Meteorology, Bratislava. *Denmark* : Meteorological Institute.
Finland : Institute of Meteorology, Helsinki University.
France : Academy of Sciences, Paris.
Germany : Scientific Academy of Berlin ; Meteorological Service, Democratic Republic ; Zonal Meteorological Organisation in the U.S. Zone.
Greece : National Meteorological Service, Athens.
Holland : Royal Netherlands Meteorological Service ; Netherlands Meteorological and Astronomical Society.
Iceland : Meteorological Office, Reykjavik.
Israel : Meteorological Service.
Italy : National Academy dei Lincei, Rome ; Institute of Geophysics.
Norway : Meteorological Institute.
Poland : Hydrological and Meteorological Institute, Warsaw.
Spain : Royal Academy of Science and Art, Barcelona.
Sweden : Meteorological and Hydrological Institute.
Switzerland : Physical-Meteorological Observatory, Davos-Platz.
Turkey : Meteorological Service.
U.S.A.: American Meteorological Society ; American Geophysical Union ; Weather Bureau, Washington.
U.S.S.R.: Academy of Sciences, Moscow.
Yugoslavia : Hydrometeorological Service of the Federal People's Republic.

The President then announced, amid acclamation, the election by Council of five Honorary Fellows :—

Dr. S. K. Banerji	(India)
Dr. R. Bureau	(France)
Dr. H. Ficker	(Austria)
Dr. F. W. Reichelderfer	(U.S.A.)
Professor Ir. F. A. Vening Meinesz	(Netherlands)

The following awards were then made and the citations read :

(a) BUCHAN PRIZE, in respect of the period 1944-1948 (*Q.J.*, **76**, p. 250)
To Charles Henry Brian Priestley, M.A., F.Inst. P., (*in absentia*).
To Reginald Cockcroft Sutcliffe, O.B.E., Ph.D.

(b) HUGH ROBERT MILL MEDAL AND PRIZE : (*Q.J.*, **76**, p. 250)
To Hugh Robert Mill, D.Sc., LL.D., (*in absentia*).

(c) DARTON PRIZE : (*Q.J.*, **76**, p. 251)
1. Rodney B. M. Levick.
2. James M. Craddock, M.A.

Professor Ir. F. A. Vening Meinesz then delivered the Centenary Address, following, and the meeting terminated at 4.30 p.m.

551.5(04) : 33

The meteorologist serving the community

By Professor Dr. Ir. F. A. VENING MEINESZ
Chief Director of the Royal Netherlands Meteorological Service

An address given before the Royal Meteorological Society on the occasion of its Centenary on 8 April 1950

It is a great honour for me that the Organizing Committee for the Centenary Celebrations of the Royal Meteorological Society have requested me to address you on this memorable occasion of the Centenary Meeting, but although I have from the very first highly appreciated this great honour I have seriously hesitated whether I ought to accept it. My own line of science is neither meteorology nor climatology so that I am not able to deal with these sciences here in the expert way which this great occasion demands. And I should very much have liked to do so because meteorology has developed so strikingly in the last hundred years that a good survey of its growth and of the outlook for the future would not only be worth while but it would also be the right time and the right place to undertake it ; justice could also be done thus to all that your Society has contributed to this development.

I *have* undertaken to speak here, notwithstanding these considerations, because there is another subject which offers itself for which I feel myself better qualified, *viz.* the spectacular change in the position of meteorology and meteorologists with regard to humanity as a whole — in its national as well as in its international aspects. I do not say that I am able to do justice to this very big subject but I hope I may succeed in engaging your attention in it during the short time that is allotted to me. In order to do this I shall place it against the background of the stupendous changes which the human population of the earth's surface have undergone in recent times.

It is one of the happy sides of a Jubilee Celebration, like this Centenary of your world-famous Society, that it leads to a moment's pause on our road, allowing us to look round, to look backward and forward, to compare and to reflect, to take distance from the circumstances surrounding us during our breath-taking advance in this age of hurry and to assess our position with regard to our science, to our national community and to the international community of nations. And if we do so for meteorology we find, perhaps to our own astonishment, how profoundly and how quickly this position has been modified in the last ten or twenty years.

In this regard two general lines in the development of humanity deserve our attention : first, the enormously increased significance of the natural sciences and their technical application for the welfare and well-being of human society and for national and international relations, and second, the fantastically accelerated development towards internationalization of the different peoples of the earth.

In both directions meteorology plays an important part and it stimulates our enthusiasm for and our devotion to our profession to realize this and to play this part consciously and well-directedly.

There can be no doubt about the first point I mentioned, about the increased significance of science and its application to human effort in all directions. Let us take for instance the physicist in general. Before the last war the number of young physicists who had finished their university studies was often larger than the number of posts open to them. In many instances they had to accept positions more or less outside, and often below, their capacities and abilities. Now the situation is entirely changed. The demand for the able young physicist far exceeds the number available from our universities and institutes of technology. In several countries, as *e.g.* in England and the United States, this is so well appreciated by the governments and in leading circles that special measures are taken to increase the number of students in this and other branches of the natural sciences. There is scarcely any branch of industry nowadays which has no research department for the development of new methods and new possibilities, and for widening in this way the scope of its activities. New discoveries lead to new articles and new facilities for man which may perhaps in the beginning be looked on as luxuries but which are often so quickly absorbed by the people in general that in a short time they are considered to belong to the normal standard of living to which everybody wants to adhere. In this way industries develop to dimensions that would be incredible in former periods, periods that in many cases are quite recent. The car and the radio are just two striking examples, but everybody can add to them. So science has modified our whole world.

This process, it is a platitude to say it, has of course been going on since the beginning of last century but it has accelerated continuously and is now developing in such a way that the number of scientists engaged in it has enormously increased. Before concentrating on what this means for meteorology I shall continue for a few moments to consider the physicist in general in order to deal with the most spectacular side of the process, *viz.* that connected with the development of nuclear physics. And I shall discuss this especially with regard to the great moral problems it has raised for the scientific worker, problems practically unknown till before the war, and which now threaten not to remain limited to that field but to invade also other domains of the natural sciences ; they may well come to the meteorologist also.

I need not say that I am referring to the fact that during this last war a great number of physicists were requested to co-operate with the military authorities for developing the atom-bomb, that is to say that they were asked to develop the new discoveries in nuclear physics for application to warfare. It is well-known that this development has since been continued and that it has led to still more devastating possibilities. It is hardly an over-statement to say that in the near future, if not already at this present time, it will be possible to create horrors of annihilation of such an overwhelming extent that the whole of human civilization will receive an extremely serious set-back if not a downright death-blow. That this fantastic development, which, moreover, not only threatens us through the new discoveries of nuclear physics, but perhaps also through chemical or bacteriological warfare, brings the scientist face to face with moral problems of overawing gravity is clear. And, in view of the fact that one of the most serious dangers brought forward by the military application of nuclear processes consists in the huge clouds of radio-activity that may be created, I am afraid that the meteorologist will not escape these horrible responsibilities ; a forecast for one or two days ahead of a certain wind direction may play a decisive part.

And so we may all be confronted by these grave problems which in fact remind us of the remarkable actuality of the Bible story of the Lost Paradise. The " tree of knowledge " which up to now has shed so many and prodigious benefits on humanity, has now produced fruits so dangerously poisoned that they may mean the final downfall of man.

I shall not attempt a long discussion about these moral problems troubling the modern scientist, nor shall I try to give a solution ; it far outranges the capacities of one man to do so. But I may give a humble opinion. To my mind it seems that no general rules for the decision of the scientist can be given and that it must be left to each individual personally to decide as his conscience tells him to do. But a few points appear to me to be fundamentally certain.

In the first place I think that no doubt ought to be entertained as to the right of the scientist to develop his science even in case he sees that possibilities of application greatly detrimental to the interests of humanity may be the outcome ; with this reserve, however, that this *application* is not the aim and purpose of his efforts. The responsibility of the use and mis-use of his results rests, in my opinion, solely with those who direct this application and not with the scientist who by the creation of his results brings about the possibility. But it seems equally certain to me that he can not claim the right, as a number of scientists have done in recent discussions, to publish his scientific results if the authorities in his country oppose it in the interest of the community. We are happy to live, you as well as I, in a democratic country, where we have a government deserving the loyal support and trust of its inhabitants, and we must leave it to this government to decide in case of doubt whether publication can be allowed. This of course is an encroachment on the duty of publication which each high-minded scientist feels as an obligation to the principles of freedom of thought he cherishes, but I do not believe he can vindicate this right if other high principles oppose it and if his government after serious and loyal consideration decides against it.

The question whether the scientist ought to co-operate in developing war applications of his or others' scientific findings, is a much more difficult one to answer and here I think that, as I have already mentioned, each scientist has to follow his own conscience ; he certainly ought to be free to do so. I personally hope that his decision will always be in harmony with the high directives Christ has given us and I pray that His Spirit may guide us all in the critical age we are living in.

But I have already given too much time to these grave and world-encompassing problems which no doubt may directly and seriously involve also the meteorologist but which are at this moment outside our immediate present task. I want now to examine the great contribution meteorology itself is giving to the development I have discussed ; there can be no doubt it is in the first rank in this general process of putting science at the service of the community.

Let us consider in the first place the weather service and its great improvement in recent times as brought about by the increase of data, *e.g.* those of the upper atmosphere and those of the atmosphere over the oceans, and by the better handling of these data due to the important development of dynamic and synoptic meteorological theory. I am not in a position adequately to deal with this subject but I ought not to mention it without at least doing homage to the Norwegian leaders in this field who gave such a great impetus towards new development.

There is no doubt that this great improvement of the weather service has enormously enhanced its significance for the community ; it is hardly possible to exaggerate this. How great a part the weather forecast plays in the daily life of everybody nowadays. And besides its value to individuals I may mention its importance for general activities, for festive open-air gatherings, for sport, for sailing, for all out-of-door matches and meetings. Here again we may say that if in the past a weather forecast was perhaps considered in the light of a luxury, it has now developed towards being a daily and hourly need which nobody should want to miss. It has brought about a frequency of distribution of forecasts which a short time ago would have been regarded as exaggerated.

But the forecast has also become a necessity for numerous branches of human activity of great economic importance. It is commonplace to point to aviation for which meteorology is a life question. It is of course this aspect of the weather forecast which has been largely responsible for the great extension of the national meteorological services in the last one or two decades. In the U.S.A. the air-line companies have even developed their own meteorological services in addition to what the Federal Government has done. To be well briefed may be literally a matter of life and death to the pilot of the plane and his passengers and crew and so great responsibilities are in question here. We can thus easily see the need for considerable governmental expenditure for this purpose. And besides the security side the economic significance of briefing plays a great part in air navigation ; the choice of the most advantageous route and altitude has an important bearing on the amount of fuel needed.

The great cost of modern meteorological services at a time when many nations are in such difficult circumstances lays a heavy duty on their directive officers. In order to raise the financial output as high as possible they have carefully to examine in what way they can bring these services to the fullest possible use to other economic interests as well.

Next to aviation I should refer to agriculture and horticulture. It is probably true to say that the financial interests in these industries are just as large or even larger than those involved in air navigation. In the Netherlands we have, during the last few years, experimented with special forecasts for farmers and horticulturists and the results are spectacular. Notwithstanding the conservatism for which the farmer has a reputation, listening-in to these messages has developed " like wildfire." I do not think I exaggerate if I say that nearly all farmers are at present tuning in to these forecasts. The great economic importance of this service may perhaps justify giving a few details.

For giving these forecasts telephone contact is maintained daily with two other services, namely, the agricultural advisers appointed in each province by the Ministry of Agriculture, and the Phyto-pathological Institute of the Agricultural University at Wageningen which studies plant-diseases and plant-enemies. Both institutions thus collaborate with the weather service for preparing the advice which, under the prevailing weather conditions, is to be given to farmers and horticulturists. This may be, *e.g.* advice for spraying fruit-trees against a special insect when the critical time for attack has come, or it may be advice for taking young plants out of the hot-beds or glass-houses and planting them in the open field, or it may be advice for haymaking, and so on. As I have already mentioned, these forecasts, given at hours suitable to farmers and horticulturists, *e.g.* at 0545, 0645 and 1230, have quickly reached an astonishing degree of popularity although hardly any special measures for advertising them have been taken. This is a clear proof of their usefulness. In order to ensure that this special weather service meets all requirements a commission has been formed in which the three institutions concerned meet from time to time with other representatives of agriculture and horticulture.

It is hardly necessary to point out to this assembly the great many other ways the meteorologist or climatologist can serve the interests here considered. Research into and forecasts of night frosts are of course of foremost importance. The same is true of plant-diseases connected with weather and climate, as *e.g.* phytophthora and many others, for research about the best hot-house climate for a special cultivation, for studies of micro-climates connected with different crops and the means to influence them favourably as may be attempted, *e.g.* by planting hedges at specified distances, for research about the best sowing or planting-time in various parts of the country and so on. The number of these studies may obviously be increased indefinitely ; we may be sure that agricultural meteorology is yet in its infancy. The economic significance of this branch of our science can hardly be over-estimated.

And for how many other fields of human activity is a study of meteorology and climatology important ? Fishing and shipping are two subjects which I cannot deal with here, even in a cursory way. There is also engineering, which in many ways is dependent on the weather. For the repair and the final closing of the dikes of the island of Walcheren, destroyed in many places during the last phase of the war, continuous advice from the weather service about the state of the sea to be expected has been of decisive importance. Frost forecasts are indispensable in connection with all casting of concrete and, in general, for all masonry work. And the list can be continued with many other instances.

Frost forecasts are, moreover, valuable for nearly the whole of the human community. In many cases great economic interests are involved. The prediction of an oncoming severe frost may allow the timely gathering of stores and supplies, *e.g.* potatoes and many other articles. The forecast of no frost during the week-end in winter-time permits companies and services to put out the fires of central heating installations and thus to save large sums of money.

With the exception of the extreme case of radio-active clouds I have not yet mentioned the importance of meteorology to the army and navy in times of peace and war. We have in the first place the great need for meteorologists for military aviation which in war assumes great dimensions and which thus warrants in itself the creation of an extensive meteorological organization in each country. But there are other military activities where the meteorologist's collaboration is required, *e.g.* for ballistic information to the artillery. Another and spectacular case occurred during the last war when some well-known meteorologists, whom I here salute, bore the overwhelming responsibility of the meteorological advice needed for D-day operations. And it is hardly necessary to point out the great part played by the meteorologist as well as the oceanographer in many naval operations. So the great demand for meteorologists in war-time is an established fact and practically all meteorologists will, during war, have the satisfaction of being able to serve their country by activities which reflect the great importance their science has, under all circumstances, for the community.

The impressive list I have given of instances in which the importance of meteorology can be put forward is not only incomplete but I am sure that I have not been able to do justice to them ; I must ask your pardon for all I have omitted or understated. Even in this imperfect shape, however, one cannot fail to be convinced of the outstanding services meteorology is rendering to the community.

There can be hardly any doubt that the importance of meteorological services will grow further in the future. We may expect that gradually, with an increasing amount of data, including the upper air, and a further-developed understanding of the processes in the atmosphere, the uncertainty adhering to the forecasts will become less and less. We may also have some confidence that along similar lines we shall succeed in extending the forecast over more days in advance than is now possible. In the third place we may hope, although this appears a bolder expectation, that the foundation of long-range forecasts will become more reliable than is at present the case. There are certainly some indications that the statistical research made for this last purpose will gradually reveal more than is now available of the physical background hidden beneath the correlations obtained ; a severely critical study of the significance of these correlations must certainly accompany this research. And lastly I may mention the research done in order to bring about rainfall in areas where this is especially needed ; the outlook in this direction is no doubt hopeful although the practical feasibility in many cases seems as yet uncertain. It is not necessary to stress here the importance of all these lines of advance for the further development of the services meteorology can render to humanity.

I should now like to finish by discussing another direction in which meteorology plays a part ; in the beginning of my lecture I mentioned its contribution to the internationalization of humanity. This last subject is a very great one which belongs to world history and I feel like a humble spectator who only sees a few strands of a magnificent and grand tapestry when I try to analyse what is going on around us during this age of unparalleled human suffering and equally unparalleled grandeur.

If we study history it becomes clear that humanity in its development towards always greater units of organized government, from township to county, from county to nation and to-day from isolated nation to supernational groups, always did so when the growth of its technical abilities gradually enlarged the areas of its normal social and commercial intercourse until the old form of organization no longer fitted its needs. And we strongly get the impression that in the present time we are contemporaries of another such development. But I think two sides of this process are surprising and in the highest degree interesting.

In the first place it is astonishing to see that humanity in its present state of high civilization and of great organizing power has until now suffered wars as the only way to settle the differences between nations although we all know their barbarous character and the ineffectiveness and injustice of the solutions they often bring about ; humanity has been powerless to create better ways of settlement.

In the second place it is, I think, a revelation to see that the change to greater units of organization which abolishes wars inside these units and which thus has brought us so much nearer to the ideal of one world without any war, that this change has been brought about not through human wisdom and human goodness but through a mysterious development which has mostly proceeded in spite of the plans and actions of the governing statesmen and the houses of representatives of our time. On the contrary this development has been accompanied by more devastating wars and by greater cruelties than have ever occurred in human history.

I am deeply impressed by this march of events in which we see humanity, in the grip of its own scientific and technical creations, developing towards a growing unity, which has led to-day to only two groups, although, alas, not yet to the final step of entire union of all the populations of the earth. Have we to see this development as a victory of the Spirit which went its way independent of the small strivings and petty plannings of humanity ?

I shall not try further to penetrate this august mystery but I think that we are entitled to at least one conclusion. It seems that all human action in harmony with this tendency towards unity of the whole of humanity is to be encouraged with all our strength. Many of us, moreover, feel such action to harmonize with their deepest convictions.

I shall not try to put this into words. Let us be glad that this line of action does not clash with our life-position : among all scientists the meteorologist is particularly favoured in that his work tends more than any other's to international collaboration and international bonds. No meteorological research could be satisfactory without accepting this standpoint. There is no other science of which the study and the application is leading to so intimate international collaboration. The organizations of the International Meteorological Organization, of the meteorology division of the International Civil Aviation Organization, and of the International Association of Meteorology are there to prove it.

I am getting near the end of what I wished to speak about here. I have expressed my deep conviction that the meteorologist, although his life is often a hard one because of the rigid discipline and the necessity for keeping night watches, is favoured above many other scientists because he serves humanity in two ways. He serves it in the national community because of the great significance his work has for its general welfare and well-being. He serves it in the international community because of the great contribution he makes to international collaboration, understanding and friendship.

To the development of meteorology and to the development of this noble side of it, your great and famous Society has for one hundred years devoted all its energy and made many contributions. It is a great honour for me that you have allowed me to-day to bring to you and to your science this high tribute.

I thank you for the attention you have been so kind as to give me.

Centenary dinner

The Centenary dinner of the Society was held at 8 p.m. on 3 April 1950 at the Connaught Rooms, London. Some 150 members, their ladies, and guests were present.

The Toast list was as follows :—

THE KING

Proposed by – Sir Robert Watson-Watt, C.B., F.R.S.
President, Royal Meteorological Society

HIS MAJESTY'S MINISTERS

Proposed by – Sir Robert Watson-Watt, C.B., F.R.S.
Response by – The Rt. Hon. Arthur Henderson, K.C., M.P.
Secretary of State for Air

THE PRESS

Proposed by – Sir Nelson K. Johnson, K.C.B., D.SC.
Director, Meteorological Office
Response by – W. F. Casey, Esq., LL.D., *Editor, "The Times"*

OUR GUESTS

Proposed by – E. Gold, Esq., C.B., D.S.O., F.R.S.
Response by – Professor Ir. F. A. Vening Meinesz
Director, Royal Netherlands Meteorological Service

THE SOCIETY

Proposed by – Sir Robert Robinson, O.M., F.R.S.
President, The Royal Society
Seconded by – Dr. Sverre Petterssen, *Weather Bureau, Washington*
Response by – Sir Robert Watson-Watt, C.B., F.R.S.

Sir Robert Watson-Watt proposing the toast to His Majesty's Ministers :

My Lords, Ladies and Gentlemen,

I propose with the tolerance of our honoured Guest, the Secretary of State for Air, to address to the Minister for Meteorology a few remarks on meteorology and politics or if he prefers it, meteorologists and politicians. I ask to be allowed, Sir, to resume for the moment the mantle, but not the responsibilities, of your Scientific Adviser. It is my present intention to divide my remarks into three major sections, the first to deal with the politics of meteorology, the second with

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the meteorology of politics, and the third, which indeed falls into both these classes, on the relations between the Royal Meteorological Society and the State.

Coming to the first category, the politics of meteorology, I trust, Sir, that our staff work was not lost on you. We arranged a message about a motor-car . . . . so that you might have a clear demonstration of the law-abiding character of your professional staff in the Meteorological Office. The car, so far as I can ascertain, does not belong to any meteorologist. Comparatively few meteorologists are, in fact, in a position to . . . . (laughter). I don't know, Secretary of State, whether to congratulate you or commiserate with you that you have been spared the searchings of conscience which racked the minds alike of the President of the Board of Trade and the then President of the Royal Society in the vexed days of 1860, when there was a somewhat heated discussion on whether His Majesty's Government should "take the responsibility of drawing conclusions." It was recognised that it was reasonable for them to issue statements of meteorological fact but the question of whether they should do anything about it was debated with some heat. The problem was solved, I think in a characteristic British fashion, just for a short time by the ingenious evasion devised in 1861 by which the storm warnings, which were already being issued in quantity, were in fact issued at Government expense, but were not regarded as Government advice. In fact, the relevant minute said "The signals are the signals of Admiral FitzRoy and not of the Board of Trade." But that was not the whole story. In 1866 the battle still raged, if you can ever call a battle in the Treasury a thing that rages, it is much more of a cold war than that). But in 1866 they issued from the Treasury a horrific *cri de coeur*, which referred still to this nelsonically insubordinate Admiral FitzRoy. It said "The whole system of telegraphic warnings had been organised in opposition to direct instructions from the Treasury." We have gone a long way from that in the present day, Sir : you would not permit the Tressury to issue instructions in matters of this sort. The doctrine of managerial responsibility in the individual Department is at least established in principle, if not very firmly in fact. And then we come to the happy ending ; in 1874 — you will observe it had only taken 14 years — it was officially admitted that storm warnings had "been forced upon us by the public." I wonder, Sir, whether you have ever since regretted that the decision took the form it did, or whether you have wished at least that the President of the Board of Trade had retained his reponsibility for storm warnings elsewhere than in the film industry.

The question of the relation between meteorologists and politicians is one which I find of personal interest, although I haven't counted the army of transitory Ministers, if you will forgive me, Sir, but Ministers *are* transitory — the civil servant is well dug in — I haven't reviewed the Ministers under whom I have had the privilege of serving. I remember amongst my early Ministers the great philosopher A. J. Balfour, but even so he came comparatively late in my chequered civil service career ; when I joined the Meteorological Office we were a curious institution operating quasi-independently under what is called a grant-in-aid from the Treasury. Now I am very sorry that the two science graduates who have adorned the Chancellorship of the Exchequer, have been denied by your intervention, Sir, or by that of your predecessors, the privilege and the fun of running a scientific department of their own under a grant-in-aid. Meteorology owed a great deal to the Board of Trade and a great deal to the Treasury in those grant-in-aid days, when Napier Shaw was laying the foundations of a good Office and a good science. But it is not only to the Departments, including now the Air Ministry, which have administrative responsibility, it is not only to them that the science of meteorology is greatly in debt. The Department of Scientific and Industrial Research has made its own contributions. Ag-and-Fish, as it is affectionately if a little irreverently called, has done a great deal for meteorology. The Post Office has made its contribution. The scientific development establishments serving the Defence Departments have made a great many direct and indirect contributions to meteorological technique.

In my own field of radio the whole elaborate network of telecommunications which are required by the meteorological services of the country, the provision for facsimile transmission of weather charts, the channels for the reception of radiosonde data, the aids that radar can give you ; all these have been contributed by other Departments, and to the Ministers concerned, we, on this our hundredth birthday, record our gratitude.

I must, Sir, pass to the second head of this typically Scottish sermon, the meteorology of politics. There is a great deal in common between the meteorologists and the politicians. The meteorologist is largely concerned with opinions on climate, the politician is very acutely interested in the climate of opinion. We are both profoundly concerned about the relative stability of world situations. And after the symposia at Oxford I have an uncomfortable feeling that the meteorologists know almost as little about the relative stability of world situations as the politicians do. We are both concerned with the early detection of incipient disturbances. We have a long-range ear which listens for thunder on the left. I think perhaps at the moment, Secretary of State, your colleagues may not need such a long detection range as we require in meteorology. We have radar devices which give us warning of approaching squalls, but you would share with us our disquiet at the relatively short warning that these devices give us. We both find some value and some interest and a good deal of puzzlement in the result of the launching of our *ballons d'essai*. The politicians have (not in your special sphere, Sir, but more generally) had occasion to recognise the regrettable fallibility of forecasts, whether they were made in November 1949 or in February 1950. And I would like, more seriously, Sir, to address you on the principles of that science which you guide and adorn. There lies at the root of our synoptic meteorology a thing called Buys Ballot's Law, ascribed (and we don't quarrel with 50 per cent. of the ascription) to one of our continental colleagues. In actual fact it was of course a Scotsman who really got to the bottom of the business, and I think we ought to celebrate our centenary by rechristening the law, if it's not too late, the Buys Ballot-Buchan Law. Anyway, the law, if I may put it in the language of section 2 of my commentary, runs this way : " If you are drifting with the current, then you are acting under the influence of high pressure from the right." I wouldn't, of course, know, Sir . . . . I think perhaps discretion suggests that I should look at my watch and proceed to section 3 of my address.

The Royal Meteorological Society is fortunate in drawing a very large part of its strength from the fact that a great majority (almost the whole) of the professional weather men in the Meteorological Office of the Air Ministry and of that slightly isolationist department, the Admiralty, are in membership of the Society. The quality of our scientific proceedings and the high international reputation which has been achieved by our *Quarterly Journal*, are in very large measure dependent on this strengthening of our otherwise perhaps slightly amateur thinking by the pure milk of the official gospel of meteorology which flows through these channels of membership of the Society. But it is not one-way traffic. I know that Sir Nelson Johnson feels that he and his staff in turn owe a very considerable debt to the Society for the opportunity which they have of taking their interim conclusions (there are no conclusions but interim conclusions in meteorology) and indeed taking their half-formed thoughts into the forum of the monthly meetings of the Society and having them discussed in a friendly, informal but certainly critical and sometimes sceptical atmosphere, by their peers inside and outside the Office. I believe, Sir, that this is the very happiest of complementary relationships. I am sure it is better than the state we might have reached had the advice of a misguided servant of the First Lord been taken in those days when, apparently, politicians talked quite a bit about meteorology and not just about the forecasts. 1877 was one of the critical dates in the history of the politics of meteorology and a very senior officer in the Admiralty then said " Everyone has a finger in the less exact science of meteorology," — the Astronomer Royal will note with pleasure that the more exact science under reference was astronomy — " and I shall be very sorry for the Hydrographer to have, in addition to the responsibility of the Hydrographic Office, half a dozen societies of amateurs in all parts of the Kingdom teaching him his work. If it were possible that it could all be done by one Government Department, I think it would be very much better." Well, their Lordships seem to have forgotten about that, it is not yet being done, even by one Government Department, even the bit of it that is being done by Government Departments.

The Society also, I think, does some good for the Meteorological Office and for you, Secretary for Air, in your position of accounting for the expenditure of the Office, by providing a channel of wide publication for the more austere work of your Meteorological Office. Perhaps I might be allowed to whisper, since you have the President of the Royal Society so closely at hand, that you and he might consider whether that fact, that we are the channel of publication of a large part of the official scientific product of the Meteorological Office, does not entitle us to just the merest trace of consideration in the administration of the grant in aid for scientific publications which is so wisely and benevolently (but cautiously) dispensed by the President of the Royal Society.

And since I have now reached the Scotsman's favourite topic of economics, I would refer to Friday morning's symposium at Oxford, when Mr. Hagen gave us a very lively address bearing on the work of the " Seventeen-cent Weather Bureau " of that nation on which we are dependent for much friendship, much mutual support, much wisdom, mingled with a small amount of folly as all human wisdom must be, and I would respectfully call your attention, Sir, to the fact that you have a ten-cent Meteorological Office. I feel quite sure that you would have the support of the science graduates in the Cabinet, and of the particular science graduate who is (with yourself) most closely concerned with this matter, if you suggested that for the moment at least, he might send up the vote for meteorology at large in this country first of all to parity — parity is well understood in these discussions — and we could get along for a year, I think, Sir Nelson, on 17 cents in place of the 10 cents per annum per head of population which we now enjoy, if enjoy is the right word. I believe that we meteorologists give very good value for our 10 cents and I would hope that as a result of the friendly attention which our guests of the press have so handsomely given us in the course of the last week, you might even carry your demands, with proper graduation in time, to a level where we might hope for a forty-cent Meteorological Office and then, Sir, you will get something : you get something pretty good now, but you'll get something a great deal better ; indeed it would be a much better investment.

The very pleasing duty, which falls to me now, Sir, is to ask the Company to drink to the health of our wise masters, very faithful servants and preposterously over-worked slaves, His Majesty's Ministers !

THE RT. HON. ARTHUR HENDERSON, K.C., M.P., *Secretary of State for Air*, in response to the toast of HIS MAJESTY'S MINISTERS :

MR. CHAIRMAN, MY LORD, LADIES AND GENTLEMEN,

I must confess that I have a very queer feeling as I rise to speak in this gathering, the one solitary humble politician in the midst of some of the leading scientific minds in the world to-day.

But first of all, may I express one word of congratulation to the Royal Meteorological Society on this very auspicious occasion. After all, Sir, you and I, I think, when we reach our centenary will perhaps be grateful that life has been given us for a hundred years. Similarly those who are the members of this particular Society can take great pride in the fact that their Society has carried out its activities during that span of years.

Sir, you spoke of the various divisions of your speech, of the politics of meteorology and of the meteorology of politics. Well, I don't know anything about the politics of meteorology, indeed I don't know anything about meteorology. But I do wish that it were possible to do in the realm of politics the same sort of work that is done in the realms of meteorology. You have very tactfully referred to the political situation that exists at the present time. As I say, I don't know anything about the weather barometer, but I do know a little about the political barometer. And the political barometer at the present time is very unsettled. When we are dealing with the weather barometer, we can turn to the meteorologist to give us his forecast. It isn't always correct, but still, we do get a forecast, and it is put over by the BBC, and is a very great assistance to everybody.

Well, now, Sir, I wonder whether it wouldn't be possible for the BBC to put over a kind of political forecast. For example last Wednesday, I think that such a forecast would have been extremely useful to everybody. I would like to suggest this as a very useful forecast :

"A ridge of high pressure is moving over from right to left. Sudden squalls are expected to be followed by long periods of settled conditions on the left with continued depression on the right."

Well, Sir, if I may be allowed to say so, that was the first time that I have personally ever attempted to draft a meteorological forecast.

Now, Sir, meteorology, as you have said to-night, is one of the most important sciences of modern civilisation and indeed it is a science that will become increasingly important. Nevertheless, you know better than I do that it is a comparatively young science. Although a great deal of development has taken place in the past 100 years since the foundation of your Society, I think we shall be agreed that there is still a great deal of ground to be covered before weather forecasting can be classed as one of our exact sciences. Nonetheless, weather forecasting is a social service which caters for the needs of John Citizen in almost every form of national and industrial activity, agriculture, shipping, industry, sport and the family holiday-maker. But, Sir, in my humble opinion it is perhaps in the air that accuracy of meteorological information is most vital. The advent of jet-aircraft, the advent of the jet-airliner, capable of speeds approaching eight miles a minute and of flying from continent to continent where weather conditions may be widely different, make it imperative that weather forecasting should be accurate and detailed over very great distances. Safety and speed must keep in step. You know, Sir, better than I do, that very great strides have already been made in this direction and I sincerely hope that the week's activities will have shown those of you who are present here to-night, who may have been sceptical before, that the Meteorological Office is in the forefront of modern development.

A recent check of forecasts has shown that an accuracy of 90 per cent. is being achieved in the London area and this is no mean achievement. It is true that this figure relates to forecasts of rain and it may be that, English weather being what it is, a forecast of rain can hardly be wrong. But farmers would, I think, take a different view and they at any rate fully recognise that the forecasting service provided by our Meteorological Office is an indispensable part of their farming operations. There are those, of course, who rely on pains in various parts of their anatomy — I believe the big toe is a favourite choice — to tell them when thunder or rain is on the way, but I think they are in a falling minority. In any event, it is my opinion that the pains in question are not due to external humidity but rather to internal humidity.

Sir, to sum up, I should like to assure those of you who have the future of this science at heart, that, and this may be some consolation to the Chairman, although I am afraid I am not going to be able to translate what I am about to say into terms of cents or any other currency, that meteorology ranks high in the priorities to which our limited resources — and I not only mean the Air Ministry's limited resources but those of the nation — are now devoted. And I, Sir, am confident that the service which the Meteorological Office provides for aviation, for industry, farming and for the general public will continue to improve and that its vital importance in the modern scheme of things will be more generally recognised as time goes on.

Sir, co-operation between nations in the realm of meteorology shows how well the needs of humanity can be served as a result of real international co-operative effort. If only the same measure of co-operation could be achieved in all aspects of international relations, the dangers now threatening our world civilisation would be replaced by an era of fruitful advancement and security for all nations.

SIR NELSON K. JOHNSON, K.C.B., D.Sc., *Director, Meteorological Office,* proposing the toast of THE PRESS :

MR. PRESIDENT, MY LORD, LADIES AND GENTLEMEN,

With your permission, Mr. President, I should like to take this occasion of expressing to the Press on behalf of your Society, our appreciation of their co-operative attitude in publicising the Centenary Celebrations.

The question of our relations with the Press has set me wondering and the cause of my wonderings has been this — for some time past, it has become increasingly apparent that the public has become more meteorologically conscious, and I wonder what we are going to do about it. I wonder whether the meteorologist has not some kind of duty which he owes to the community to meet that consciousness of meteorological matters which has gradually developed in the community.

The meteorologist is apt to shut himself up in 49 Cromwell Road, to talk a lot of jargon to his colleagues and then, to put on his hat and coat and go home. I wonder, hasn't he forgotten that there is a public who would be interested to know what he is talking about, what he is thinking about.

You say, we have a very excellent little journal called " Weather " and I entirely subscribe to that. I think those responsible for editing it are deserving of the very highest thanks and admiration from all. But the circulation of " Weather " runs into thousands, while I am thinking about millions. What are we going to do about the millions ? And I am wondering, are we as meteorologists too modest ? Or are we afraid of being accused of self-advertisement ? Or again, are we nervous of Press distortions ? Ought we not to take the initiative ourselves and expound the truth by our own words instead of leaving it to other people to present the truth at second hand ? Or are we perhaps unduly conscious of our own limitations and the limitations of our science ? In that connection I would like to interpolate that I am not thinking of limitations in forecasting only ; I would like to suggest that the non-forecasting help which the meteorologist can give to the community is at least as great as that which can be given by forecasts.

To return to my theme, what are we going to do about it ? At one of the discussions at Oxford, one speaker suggested that Meteorology needed some Public Relations Officers. Public Relations Officers are very valuable people but are they sufficient ? Ought not meteorologists themselves to take the initiative and do more themselves ? Another speaker at Oxford emphasised that the public required educating. I think many of us will agree that the Press is probably the most effective medium for that purpose.

So don't you think it might be a wise thing for us to propitiate the Press by a libation ? I think it would.

And therefore, Mr. President, My Lord, Ladies and Gentlemen, may I call upon you to drink a toast to the Press with which I will associate the name of Dr. Casey.

W. F. CASEY, Esq., LL.D., *Editor, "The Times,"* in response to the toast of THE PRESS :

MR. PRESIDENT, MY LORD, LADIES AND GENTLEMEN,

The "fourth estate" is so variously composed, it comprises so many different kinds of newspapers and periodicals, that it is difficult to consider it simply as the Press ; it is like the Public, there is no such body. There are all kinds of Publics. True, they overlap, and the newspapers vary as much as the publics for which they cater.

Until very recently, they were unanimous about one thing however, and that was their views about meteorologists. (I do hope that before the bi-centenary dinner you will have changed your name, because it is extraordinarily difficult to pronounce.) They were all agreed that meteorologists, whom they regarded simply as weather tipsters, were always wrong. Now, however, many of us know that you are usually miraculously right and so the old unanimity on this point is gone. However, newspapers are still agreed on one thing and that is that anything to do with weather is News.

Some people think that some of us take our job too solemnly, others think that other papers approach it too light-heartedly, but whatever our attitude, we cannot pretend that journalism is a science. I often envy the men of science who deal not as we do with events caused by the incalculable impulses of human beings but with the calculable forces of nature. And this must give scientists a serenity that we cannot possibly attain, but I think we ought really to put in a plea for mercy in this highly complex age. We have moreover the impossible task of making scientific subjects intelligible to the plain man. I hope that what Sir Nelson Johnson said will mean that we will get a little help.

I would only like to add that all of us down in Printing House Square this morning were extremely proud that we were privileged to publish a centenary article by your distinguished President and I should like to thank Sir Nelson Johnson for his kind remarks about our trade.

E. GOLD, Esq., C.B., D.S.O., F.R.S. proposing the toast of OUR GUESTS :

MR. PRESIDENT, MY LORD, LADIES AND GENTLEMEN,

It is my duty and pleasure to propose the toast of our guests.

It is indeed a pleasure to have you with us to-night. Our science depends for its full contribution to human material welfare, and what is more important, to human culture, on the contacts of its exponents at home with their fellows from other lands, and with, do I say, brothers and/or sisters in sister societies — that is not quite right either for the Royal Society is the parent of us all and without her we should indeed be orphans, starving orphans, lacking those foundations of science on which our meteorological house is built. I am still Victorian enough to greet a parent with respect, with holy fear and with loyal affection. And I do so for the Royal Society to-night.

Next, her sister, the British Association, through which so many of our contacts with our brothers and sisters are made or renewed, and in fact, the hungry sheep of the outside world are fed. Then there is the Royal Geographical Society, and the Institute of Navigation which cannot divorce itself from meteorology and has often welcomed us to its halls. And the Royal Agricultural Society whose members make more than adequate return for all our science can do for them. The freezing of the potatoes occurs after they are out of their care, and the Minister of Transport isn't here. The Royal Aeronautical Society at whose meetings we have enjoyed some of the most fruitful discussion of the meteorological problems of the upper air. The Science Museum whose director has shown his regard for our science in a very practical way by the exhibition which absorbed our interest a week ago.

And now the Council for the Promotion of Field Studies to which we look with confidence in tracing the causes and the significance of the effects on plants and birds and animals of the integrated weather of their lives. I must also mention the schools of meteorology in the Universities of Oxford and London, and the Naval Meteorological Service, and the Meteorological Office. But it would be indelicate of me to dilate on their virtues or their duties. Their association with the Society is closer than a brother's.

Our guests from overseas are many ; they are always specially welcome. In the years that have flown I have frequently had the pleasure of engaging with some of them in the search for a formula which would combine Gallic lucidity with English workability in an international resolution. Sometimes, I admit, it has been the pain of finding them still of Virgil's mind, *ut penitus toto divisi orbe Britanni* — those British, utterly estranged from all the world. But generally we succeeded, usually at some slight sacrifice of both our aims, because in the words of Dr. Hesselberg, whose inability to be present with us to-night we greatly regret : "we are all striving to get the intrinsically best solution." May I venture to say here, now, remembering those past years of international meetings of meteorology, that I hope in the years to come, these works may be renewed with the same frankness and universality as before, and that the reservations and suspicions, and manoeuvrings, the inevitable consequences of the war may disappear. They can go only by the combination of the lapse of time with the justification of faith by works. I think we reached that stage after the first world war at the meeting in Copenhagen in 1929 after a lapse of eleven years, a not inappropriate period in meteorology. It is a special pleasure remembering that meeting at Copenhagen, to welcome here tonight the Director of the Danish Institute, Dr. Peterson.

The meeting at Vienna in 1926 undoubtedly helped to bridge the gap, and it is a cause of regret that Dr. Ficker has been unable to get here for our Centenary celebrations. We were, many of us, looking forward to renewing our contacts with him. Twenty-five years ago Dr. Hergesel brought greetings from meteorological institutions in Germany. To-day, Dr. Wenkendorf does the same, and Prof. Venturi and his colleagues bring greetings from the meteorological institutes in Italy. This centenary meeting may, therefore, assist in the direction of my hope.

In a moment or two I am going to ask you to rise, but I still have an introductory word or two for one of our guests, Prof. Vening Meinesz. I have often read of Nereids, those marvellous creatures that used to rise from the submarine and delight our ancestors with their grace, their wisdom, and their charm, but never realised that I should live to see a Nereid playing so large and so graceful a part in our science when having plumbed, or should I say, pendulummed the ocean depths, he decided to dedicate his powers to the science of meteorology. He did so at a difficult and a critical epoch. We wish him well, and now I say to him in one final lapse : *oculi omnes te expectant*, the eyes of all await upon thee. I ask you now to rise and drink heartily to this toast Our Guests, coupled with the name of Dr. Vening Meinesz ·

PROFESSOR IR. F. A. VENING MEINESZ, *Director, Royal Netherlands Meteorological Service*, in response to the toast of OUR GUESTS :

MR. PRESIDENT, MY LORD, LADIES AND GENTLEMEN,

I thank you for the honour of speaking for the Guests, but I feel it is a difficult task, especially to include also British guests. With your permission I will speak first on behalf of guests from foreign countries and of the feeling which they have. We all feel it a privilege to have been invited to the Centenary Celebrations, which brings me to a somewhat queer though true story.

We received an invitation from a lady who wished to celebrate her hundredth birthday and we were very glad to come because we knew that lady in the shape of all her children whom we very highly esteemed. Now we found out the number of these children to be about 1,500, which is rather a large number even for a lady who celebrates her hundredth birthday. We felt a great sympathy with all these children, and were very glad to come. Of course, there was something else we knew, namely, the airy — I don't speak about fairy stories — I mean the airy stories the lady had been publishing in her quarterly journal. And so we wished to be present when she celebrated her hundredth birthday.

Alas, I myself was not able to be at Oxford. I suffered from Budget troubles which are worse than a severe illness — but I won't speak about that now. I will speak as if I had had the privilege of being with you from the very beginning and had shared with all these guests of foreign countries the splendid hospitality given to them in Oxford and afterwards in London. We found at once that the lady was not an old lady, but was very young, with a great deal of vitality, and that she was starting her next 100 years quite hopefully and intended to have yet more children — which is of course getting a somewhat queer story and, I feel, perhaps not even quite a moral story — and I have to be careful because I remember a story of Mr. Pickwick who had some troubles with a lady in breach of promise — I don't remember exactly, but I think I ought to be very careful in continuing my metaphor.

So let us keep to this side of it, that we have all been very much charmed by the wonderful way your society has received us. We have confirmed that our sympathy and admiration were well founded. We have admired the way in which the festivities have been organised, and we have enjoyed to the full the scientific food which was prepared for us and which was mixed up with the festivities in so harmonious a way that each strengthened the other. We have enjoyed and are enjoying ourselves immensely. We have also appreciated the high level of scientific achievement which we have found here — not that we did not expect it, but still it has been presented to us very clearly and in such a way that we have absorbed it very easily. We have been very glad to have been here on this memorable occasion.

And so I am on the safe side of my metaphor now, when I wish this lady a very prosperous second hundred years of her life and express the desire that she will have great success in her further achievement. I want especially to thank Mr. Gold for the kind words he has spoken about us and I can assure him that all the foreign guests will return home with the memory of quite unforgettable days and of good will which has been showered upon them during these days of celebration. It has again strengthened international ties and I want to express and emphasise how strongly this favours the creation of good will among nations. I thank you very much for the kind words you have said to us.

SIR ROBERT ROBINSON, O.M., F.R.S., *President, The Royal Society,* proposing the toast of THE SOCIETY :

MR. PRESIDENT, MY LORD, LADIES AND GENTLEMEN,

I admire the modesty of the Royal Meteorological Society in the order of the toasts but I can quite understand that it is in order that your President, Sir Robert Watson-Watt, should be the alpha and omega of the proceedings.

May I add on behalf of other guests, the British ones, the very sincere thanks for your generous hospitality and for the extremely pleasant evening which we have had together.

Now, the Editor of " The Times " has referred to the fact that there was published this morning a most admirable description of the history and of the achievements of the Royal Meteorological Society in his journal, " The Times," under the pen of your President. Of course, normally speaking, I should have regarded that as somewhat cutting away the ground from under the feet of the man who proposes the toast of the Society, but this evening and at this time it is really rather a satisfactory state of affairs. I need only to refer you to that excellent article where you will find everything about the Society — you no doubt know it all already — but it is there in a very convenient form and you won't have destroyed your newspaper yet.

From that article, may I just take one little quotation, that " . . . . the relations between the Royal Meteorological Society and the Royal Society are based on the filial affection of the junior Society for its venerable senior." Now, of course, this venerable senior business is all very well providing you do not think we are at all decrepit, because we don't regard ourselves in that light at all. And in the common interest, in the encouragement and promotion of meteorological science, we say certainly " Hear ! Hear ! " to that.

The Royal Society is proud that the present-day Secretary graduated to that high office, high and responsible office, through the Presidency of the Royal Meteorological Society. He has also been President of the Physical Society, and that, of course, dotes the 'i's and crosses the 't's of the fact that meteorology is nothing but applied physics. It happens to be applied to many other things as well; in fact, as far as I can make out, everything comes into it except organic chemistry. However, that part of my remarks, the main section of what I wanted to say, must be cut out because it is rather too late.

I would only say that meteorology seems to me to be something that ranges from the physiography that I used to learn at school and which seems now, looking back on it, to have been largely meteorology, to those advanced papers, papers which include the most advanced mathematics, which is put in to give an air of veri-similitude to the narrative . . . and which often goes so far and is so advanced that it goes far in advance of your prognostications.

I also wanted to say with regard to the Royal Society that, while this is the centenary of the Royal Meteorological Society, we have a tercentenary very close to us, and that tercentenary will I think be the tercentenary of the beginnings of meteorology in this country and indeed in the world, because was it not Robert Hooke, who was one of the founders of the Royal Society, nearly three hundred years ago, who made some of the first meteorological experiments and appears to have invented nearly every one of the important meteorological instruments — it is true, he didn't invent the barometer but he put a dial on it and the familiar recording barometer of the Victorian age is the adaptation of his mechanism.

I was in the wonderful private library of Prof. Andrade recently and saw a copy there of Hooke's *Micrographia* with an illustration of his hygrometer. It seems to me most extraordinary that a man should be so observant that he knew that the hair is affected by moisture and curls up when it is dry and is distended when it is wet, and that it could be attached to a mechanism which acted as quite a good hygrometer in those days. In fact, Hooke did really discover many of these instruments, and he made observations himself on the flow of air, I believe. And it is worth mentioning, too, that a few years later Capt. Edmund Halley, the famous astronomer, also made some very important investigations of the trade winds.

Now of course, the source of my information will be obvious to you. It has already been disclosed to you that the Secretary of the Royal Society is Sir David Brunt. I have received the information from him, and he tells me that the early days of the Royal Society did see the birth of meteorology in this country.

I was going to talk about the wide scope, theoretical and practical, of meteorology, but I'll cut that out and I will go straight away to another quotation from this article in "The Times". John Ruskin, when he was 20 years of age, in 1839, described meteorology as " the young Hercules for the fostering of whose strength the Meteorological Society has been formed. Formed, not for a city, not for a kingdom but for the world." And I want to make one point and that is the very great international importance of meteorology. The fact that the co-operation of all nations is necessary in meteorological science is more obvious, I think, that in any other science. And I think there is a very great chance of bringing together the nations through a common interest in meteorology.

And that introduces another subject, namely the very great importance of meteorology in defence. I was privileged, some years ago, to serve under Sir Nelson Johnson in the Ministry of Supply and Sir David Brunt was in close association with us. We also knew in those days and of course, later, Dr. Sutton, and those three meteorologists were a proof of the very great importance of meteorology in all questions of defence of this realm.

I would like to congratulate you on the great success of your Conference at Oxford. I regret to say that I didn't attend it myself, because although I was in Oxford, I found it necessary to be in my laboratory or doing something connected with my proper job. But I understand that the scientific fare was unrationed ; I hope that it has been well digested.

Now I have already said that Sir Robert Watson-Watt is the alpha and omega of these proceedings and I would like to propose this toast of the Royal Meteorological Society, and to couple with it the name of your President, Sir Robert Watson-Watt. His very distinguished career is known to you all. You know, of course, his great services to many Government departments — the fact that he is now scientific adviser on tele-communications to the Air Ministry, the Ministry of Supply, the Ministry of Transport, the Ministry of Civil Aviation, and I have no doubt that he would act as scientific adviser on tele-communications to any other Ministry that would be interested in it. In addition, he has played a very great part in industry and as a consultant to industry.

Now, as regards his scientific work, I think that to continue my policy of quotation, which certainly shortens what I have to say, I can't do better than give you the citation which we composed at the Royal Society on the occasion when he was awarded the Hughes Medal for his very distinguished and eminent work.

" Watson-Watt has been a leader in the field of radio research since the early 1920's. He began to work on the subject of atmospherics in 1915, first on the subject of the direction of their arrival. He published the first English studies on this subject establishing the cum-solar swing of the place of origin throughout the day. He invented the cathode-ray direction finder for finding the direction of arrival of individual atmospherics. This has proved of immense value for the location of thunderstorms during the war. He worked with a number of collaborators on the subject of the wave-form of atmospherics. He has been the leading exponent of the use of the cathode ray oscillograph for a great variety of research purposes. He was the leader of the earliest British work on radar, or radio-location as it was then called. Although the basic principles were well-known, enormous technical difficulties had to be overcome before radar was an operational instrument. That these difficulties were overcome, was due in this country more to Sir Robert than to any other man."

I thought perhaps you would forgive me reading that because although it is well-known to the Fellows of the Royal Society it has perhaps not been read before to Fellows of the Royal Meteorological Society.

And I will now conclude and ask you to drink the health of your own Society, the Royal Meteorological Society, and couple that with the name of Sir Robert Watson-Watt, your President.

DR. SVERRE PETTERSSEN

MR. PRESIDENT, MY LORD, LADIES and GENTLEMEN,

I am very grateful for this opportunity of associating myself and my foreign colleagues with the proposal just made by Sir Robert Robinson. Before I justify this I would like to state my qualifications. I have had the good fortune to travel a great deal in many lands and to work with colleagues and friends in many places widely scattered throughout the northern hemisphere. During the war I had the very great luck to be accepted as a co-worker in this country under the inspiring leadership of Sir Nelson Johnson and I see around the table many friends from those days. Well, the essence of this is that I made notes and comparing these notes from many parts of the world I have come to realise and to appreciate the tremendous importance of British institutions as a source of steady progress and as a stabilising factor not only within the British Empire and Commonwealth but throughout the world. By institutions I do not mean only scientific institutions. I refer to the British institutions in general, in science, in Government, industry and defence.

The Royal Meteorological Society has acquired a position of high distinction within these British institutions and amongst us foreigners it has a position of unquestioned leadership. When I say, foreigners, I should probably better say who speak the British version of the English language with an outlandish accent.

Looking around the table to-night, I see broadly two groups of people, one being the British who are naturally proud of the achievements of our Society. If I have one grudge against the British, it is this that they have spread many things throughout the world but they have not spread the art and technique of understatement with which they are themselves so eminently familiar.

The other group is we foreigners. Now for the first group it is natural that your thoughts go back to the wise and public-spirited men who created this Society, and to the long chain of public-spirited men who have kept it going for a hundred years. For us foreigners it is probably more natural to look around the table and recognise individuals, the present generation of British meteorologists who have carried the Society to its present very high station. It is therefore, a very great pleasure and it is with deep sentiment that I wish to second the proposal just made by Sir Robert Robinson.

SIR ROBERT WATSON-WATT, C.B., F.R.S. in response to the toast of THE SOCIETY :

Sir Robert, much-loved President of our venerable but ageless senior, I thank you for the well-deserved nice things you have said about the Society and the less-well-deserved things you have said about its transient President.

Mr. Secretary Henderson, your colleague, the Lord President of the Council, was last week responsible for inciting me to a very public confession of anticipatory infidelity, which however the lady on my left had already condoned. The fact is, that I am, if I may quote with a small change a *Schlager* from *South Pacific* " I'm in love, I'm in love, I'm in love with a wonderful gal." What a wonderful woman she is ; what dress sense, what chic, what versatility. Think of her hats, from the picture hat in filmiest tulle through to the Dior velvet cartwheel, borne by a *grande dame* disdainfully conscious of the threat of rain which is of the very essence of these haloes of hers. Thence through the ospreys, the aigrettes, the ostrich plumes of her cirrus hats, the glorious gay colours of her spring boater corona, the Balenciaga saucer of the thickening altostratus, and the Aage Thaarup creation of the lenticular *Contessa del Vento*.

Consider her fabrics, the envy and despair of Jacques Fath, the *toile d'araignée* of her cirro nebula, the chiffon, the net, the tulle, the organdie, the muslin, the *organza* of her wardrobe of cirrus, the *gros grain* and *bouclé* as cirrocumulus succeeds cirrus, as altocumulus succeeds cirrocumulus.

See her costumes ; the gay little sports coats of summer cumulus, the menacing coquetries of the altocumulus castellatus, and that zenith of the *haute couture*, cumulonimbus à la Christian Dior, in which the boldly assymetric neck line in its diagonal chiffon, and the asymmetric fan blades of moire pendant from the waist, serve only to emphasise the " *Verticale* " of the dominant line. The cocktail-hour dresses of the sunsets in the new yellows and tangerines, with sun-ray pleats after the manner of Dessès. Look at the ermine cloak of her stratocumulus as seen from the sunny side, by the aircraft in flight ; see her 75 gauge 10 denier nylons in mist blue, as she casts the enchantment of her haze over our London river to make it something very dear and exceptional in our memories ; down to the micro-meteorology of her shoes, she is exquisite. Admire her satin diamanté slippers by Givre ; fear her real Cinderella *glace* slipper which still as of old may bring even a prince to his knees.

What acessories, too ; the diamanté of her dewdrops, the glittering shower of her diamond dust ; the formal elegance, the endless variety, the exquisite carving of her crystals of ice, the *grêle en cabuchon* which goes with her Dior cumulonimbus, the amusing costume jewellery " for an informal occasion " of those odd clusters of cylindrical prisms mounted on a frozen raindrop as centre.

Who but she has a complexion that can stir our souls alike with the polar blue-green of her northern sunset sky, indescribable, unparalleled, very moving ; with the pearly delicacy of her irisation ; with those more vigorous colours of her sunrises and sunsets, matching a temperament which ranges from tender gentleness through turbulence to tempestuousness. *La donna è mobile*. A George Bernard Shaw may talk reasonably of the " calculable forces of Nature," but we like, too, with Arnold Bennett, to think of our love in that immortal phrase of his, compact with devotion, admiration, puzzlement, even a trace of irritation, but always with love, " strange incalculable creature." That other great Shaw, our own Shaw, who so greatly clarified the calculable forces of Nature, clearly also loved this strange incalculable creature for her triumphant, fascinating, and inspiring incalculability.

These are the things that make every man in the Royal Meteorological Society her *cavaliere servente*. It is not that she is all things to all men ; she is such different things to such different men. She is greater than the Sphinx, for she herself gives highly intelligible answers to the highly intelligent questions which she poses — but she answers only to the suitor who rephrases them with sufficient skill. She is the *grande passion* of the Royal Meteorological Society, she is our *raison d'être*, and she unites us in a common devotion. Thank you again, Sir, for your kind words to us.

Royal Meteorological Society

On 3 April, 1850, a meeting was held at Hartwell House, near Aylesbury in Buckinghamshire, to consider how best meteorological observations should be made, recorded, summarized and published, and the *British Meteorological Society* was formed. The title was changed to the *Royal Meteorological Society* in 1883 after the grant of a Royal Charter by Queen Victoria in 1866. The Scottish Meteorological Society, formed in 1855, was incorporated in 1921. In the course of 100 years, the membership of the Society has increased ten-fold to over 1,600, the largest increases having taken place during the two world wars.

Meetings are held monthly during the winter in London for the reading of papers and more general discussions. The Scottish Centre meets regularly in Edinburgh and Glasgow and the Midland Centre in Birmingham. There is a very active Canadian Branch which usually meets in Toronto.

The chief publication of the Society is the *Quarterly Journal*, which contains original papers and reports on discussions, together with smaller articles, correspondence and reviews. A monthly magazine, *Weather*, was introduced in 1946 to meet the need for a medium of " meteorology as a humanity." It contains authoritative articles on all aspects of meteorology written in a style readily intelligible to non-specialists ; contributors include meteorologists from all over the world.

The highest honour bestowed by the Society is the Symons Medal, awarded biennially for distinguished meteorological work irrespective of nationality. The Buchan Prize is awarded biennially in recognition of the most original contributions to the *Quarterly Journal* ; and, also biennially, the Hugh Robert Mill Medal and Prize for original research on rainfall.

The membership of the Society includes Fellows, Associates, Student Associates, Foreign Members and Honorary Members. Further information about the conditions and privileges of membership are contained in a Descriptive Account of the Society obtainable from the Assistant Secretary, 49 Cromwell Road, London, S.W.7.

This volume has been prepared under the direction of the Council by the Editing Committee :

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